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by J. B. Phillips

TUNA FISHING

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From the Pacific Fisherman

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THE SCIENTIFIC MONTHLY

VOL. LXIV *March 1947* NO. 3

HARLOW SHAPLEY

PRESIDENT OF THE A.A.A.S. FOR 1947

By CECILIA PAYNE-GAPOSCHKIN

Harvard College Observatory

IN ELECTING Harlow Shapley to its Presidency, the American Association for the Advancement of Science adorns the ranks of its leaders not with one man but with many—man of science, administrator, educator, internationalist. Even as a scientist he is a multiple personality—astronomer first, but biologist too. Even those who have worked beside him for several decades have not ceased to wonder at the secret spring of the energy that flows so effectively into so many channels.

Dr. Shapley was born in Nashville, Mo., in 1885. He graduated in 1910 from the University of Missouri. It was a significant moment for astronomy, in transition from the old concepts and methods to the vistas and techniques that were to alter the aspect of the universe.

At the University of Missouri Shapley worked with F. H. Seares (later of Mount Wilson Observatory), the man most responsible for the system of photographic photometry; and at Princeton (where he went for graduate work) he came in contact with H. N. Russell, exponent and interpreter of the new direct idea of the stars as physical entities. From the years at Princeton came a formulation of the methods of interpreting the light curves of eclipsing binaries that has become classical, and the first systematic discussion of the physical properties of eclipsing stars—work in which the contributions of master and pupil were intimately fused.

With his appointment at Mount Wilson Observatory in 1914, the second phase of Shapley's astronomical work began. The opportunity was unique: the world's most powerful astronomical equipment, the new techniques of photographic photometry, and the almost equally new knowledge of variable stars—typified by the period-luminosity curve and the cluster variables. It was the imagination and industry that Shapley brought to add to these ingredients that transformed the concept of the sidereal universe. The galaxy took on its modern perspective as a result of his studies of the globular clusters and the related investigations on the nature of the intrinsic variables. The geocentric universe was gone, and its place was taken by a galaxy—formless at first, but gradually taking its shape and its place as a stellar system among many.

On his appointment in 1921 as Director of the Harvard Observatory, Shapley was still a young man, yet with the equivalent

Photograph by Science Service

HARLOW SHAPLEY

of a lifetime of research already behind him. The third and fourth phases of his work opened before him. As an astronomer, he saw his field gradually broadening outside the galaxy to embrace the distant systems that are the equals of our own in their distribution and their diversity. His study of the groupings that are supergalaxies and the gradations of the metagalaxy has led the way in the modern picture of the expanding universe, whose measured extent is beyond the power of imagination.

As successor to Pickering, it fell to Shapley to transform the Harvard Observatory, heir to a classical tradition, into an institution in tune with the transformation of astronomy. The measure of his success in what is perhaps his greatest scientific task is found in the fruits of his directorate: the diversified output of a collaborating group of astronomers and astrophysicists; the modernization and leadership in equipment and technique; the scope and number of publications; and (by no means least) the creation of a school of astronomy, with its output of the young men and women who will be the astronomers of the future. The ability to combine creative work with the many-sided duties of administration has not deprived the world of the scientist by absorbing the director, and the third and fourth phases of Shapley's scientific career have proceeded concurrently during the twenty-five years of his directorate.

To a young student who asked the secret of his success, Shapley replied that it had lain in his power of synthesis: of recognizing, and giving effect to, the interrelationships of neighboring fields. It is this power that has led him to the fifth phase of his career—his work as an internationalist. Early a moving spirit of the International Astronomical Union, he has broadened his activities until they extend far beyond the strict boundaries of the sciences and concern themselves with the graver problems of understanding and cooperation among the world's peoples.

An account of so rich a career can scarcely be enhanced by the enumeration of some hundreds of papers, articles, books, and addresses which are its outward sign. Honors are familiar to him: eleven honorary degrees, the Draper Medal, the Janssen Prize, the Rumford Medal, the Gold Medal of the Royal Astronomical Society, the Bruce Medal, the Pope Pius XI Prize, the Crux de Honor, State of Puebla, the Aztec Eagle from Mexico, and world-wide membership in academies are the index of the recognition he has earned. Nor will presidency be strange to him; he is past President of the American Academy of Arts and Sciences, of the American Astronomical Society, and of Sigma Xi.

But the enumeration of academic honors, as of scientific papers, is a superficial index of the fitness of a man to be chosen as a leader by his contemporaries. They choose him because he is the outstanding exponent of the interrelation of science and society, and because he epitomizes the qualities that they, as men of science, would wish to possess—the alert, direct, realistic response to the universe as it is and the rare power to realize and to describe it.

THE WEALTH OF THE OCEAN

By WILBERT McLEOD CHAPMAN

California Academy of Sciences, San Francisco

IN MAY of last year the Navy reported that in the course of underwater experiments with sonar instruments it had detected a layer of water off the California coast at a depth of 1,000 to 1,500 feet which showed the sound-characteristics of a semi-solid mass.

In this location it was known that there was no bottom for more than 2,000 feet below this level, and the picture on the sonar screen was not one of solid substance in any case. The matter was reported briefly in the local press, gave rise to some editorial comment about the mystery of the seas, and then passed from the public eye without its important connotations to the national economy having been appreciated.

pursed it had so many fish in it that he could not haul them all in his big seiner and had to radio to other vessels to come to his aid.

A shark fisherman came to my laboratory some time ago with two strange fish that he had captured in his net off the Golden Gate. The net had accidentally slipped into a thousand feet of water, and, while he had not caught any sharks, he had caught two albacore (which surprised him a great deal) and another species that he had never seen before.

They were beautiful hard-fleshed fish about 3 feet long and looked so much like tuna that the fisherman thought they must be a cross between albacore and some other inhabitant of the depths. They were lou-

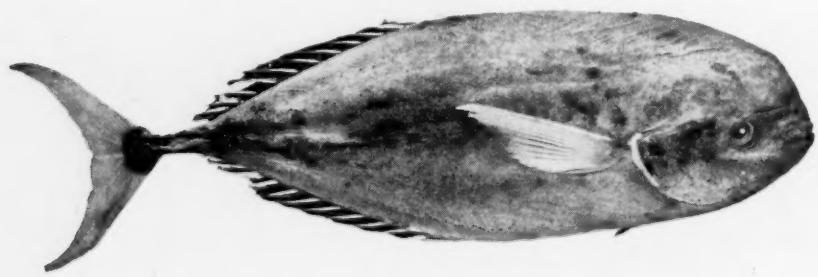


Photo by J. B. Phillips

LOUVAR (*LOUVARIS IMPERIALIS*)

A 5¾-LB. SPECIMEN TAKEN IN THE SURF NEAR MONTEREY, CALIF., NOVEMBER 29, 1939.

During the spring sardine season Vince Cardinelli was headed into Monterey with a disappointingly small catch. As his vessel ran he switched on the sonic depth-finder, and, to his surprise, the ping showed that he was over shallow water. He knew from the landmarks that this was not true and, acting on a hunch, he set his purse seine where no fish showed. When the net was

var and are so rarely taken that it was little wonder that he did not know about them; few scientists, even, have ever seen one alive. The California Academy of Sciences had none in their large collections of preserved fishes, and the number in the museums of this country can be counted on one's fingers. The louvar are tuna-like fishes of the great depths, and that is why

From the Pacific Fisherman

A CAPACITY LOAD OF SARDINES

El Padre CARRIES ABOUT 170 TONS OF FISH IN THE HOLD AND 20 TONS ON DECK.

they are so rarely seen, although occasional specimens are known from all over the Pacific, Atlantic, and Indian oceans.

I was interested not so much in the fish themselves, however, as in their tails. The tunas usually swim in dense schools, at least when they are feeding. The fish caught out of these schools have their tails frayed and worn at the tips as if the fish had been so close together that they had worn off the tips of their tails by whipping them against the other fish. When we were fishing tuna in the South Pacific during the war, we quickly learned that when we caught tuna with frayed-out tails they had come out of a school and that if we kept fishing in that spot we would get more fish. If the tails were shiny, the fish were only strays and there was no use in fishing longer in that vicinity.

The louvar brought to me had their tails worn off at the tips just like school tuna. I could not help wondering if these fish did not occur in large schools like their more common relatives that support our second most valuable fishery. This was not plausible if you believed that the waters beyond the continental shelf were barren wastes, for it takes a tremendous quantity of food to feed a school of large fish; tuna are such rapid swimmers that they burn up vast quantities of fuel.

THERE is no doubt that the layer of water discovered by the Navy off the California coast was a school of some living thing, either fish or fish food. As the evidence begins to pile up one can no longer accept unquestioningly the barrenness, from a commercial standpoint, of the far reaches and the great depths of the ocean.

One of the pieces of evidence hardest to reconcile with a belief in the barrenness of the great ocean depths is that furnished by the economy of the herd of fur seals which now lives on our Pribilof Islands in the Bering Sea. At the last census this herd amounted to something over 3,300,000 ani-

mals. We know that their diet is composed of fish, squid, and similar large inhabitants of the sea. We also know from our experience with seals and sea lions in zoos and aquariums that in order to keep a herd of seal of this size alive it would take, at a minimum, 3.5 billion pounds of fish or like food a year.

This is considerably more than the combined total weight of fish landed each year by our tremendous fisheries in the Pacific Ocean along the entire coast of North and South America from the Galapagos Islands to Bering Strait, including the Canadian catches as well as our own.

If there were any considerable competition between the fur seals and ourselves for fish, this would result rather disastrously for our fisheries, for the balance between the catch of the fisheries and the abilities of the fish stocks to replace themselves is a delicate one.

The much smaller herds of sea lions along the Pacific coast have deadly enemies in the commercial fishermen because the sea lions eat the same kind of fish that our fishermen catch. Our fishermen do not complain about the fur seal because the latter stay out on the high seas beyond where our fishermen work, and they eat fish from the deep seas which our fisherman do not catch.

But the truth is inescapably there. The fur seals are catching more food than we are out of the same ocean with us and are not in serious competition with us. This immediately leads us to ask why we do not try to compete with them and tap this vast reservoir of food for ourselves.

The reason we do not do this is our ignorance of the deep seas and of the seas more than 100 miles or so from our coast. The reason for this ignorance has been an almost complete lack of research by the Americans on the high seas of our side of the Pacific.

This is in strict contrast to the policy that the Japanese have followed. In the past 30 years they have conducted fisheries research from the Bering Sea to Antarctica, in the Indian Ocean and in the South Atlantic, seeking information which could be applied to a greater exploitation of the ocean's wealth—from the floor of the ocean to its surface.

Before the war they had at least 50 seagoing research vessels working on fisheries problems in the Pacific, and we have ac-

Photo by Victor B. Scheffer

FUR SEALS ON ROOKERY AT ST. PAUL ISLAND, PRIBILOFS

counts of 2 such vessels working in our side of the Pacific. Each of the Marine Prefectures had at least one fisheries research vessel, and some had several. In addition, there were fisheries scientists on all the large factory ships that worked off Mexico, in the Antarctic, off Kamchatka, and in the Bering Sea. There were laboratories in the home islands seeking fishing information on a scale of which we have not even dreamed.

The results were reflected in the amount of food taken from the sea by the Japanese. In the years preceding the war their catches amounted to about 16 billion pounds per year (a third of all the fish taken in the world). We were the second largest producers of fish and we took about 4 billion pounds per year.

Do not assume that the Japanese were catching some strange creatures that Americans would not eat. They were fishing for precisely the same kinds of fish that are the backbone of our own fisheries. A full quarter of the fish landed in the United States are sardines; the Japanese caught twice as many sardines as we did. The second most valuable fishery in the United States is for tuna; the Japanese landed three times as many tuna as we did. One of our largest fisheries in the North Pacific is for herring; the Japanese took four times as many herring from the North Pacific as we did. They had almost a complete monopoly of the canned-crab industry in this country as well as in their own.

This information generally goes unheeded, and we say that the Japanese are great fisheaters. But this is not the entire answer: A large part of their catch was sold in the United States, the United Kingdom, and elsewhere in Europe; their exports alone were greater than the total yield of our sea fisheries.

The reason this was so was not that their fishermen were any smarter or more hardy than ours, nor was it that they had better equipment, for no other nation can boast of

Photo by Victor B. Scheffer

A BULL FUR SEAL

as fine fishing gear and vessels as ours. It was because their fishermen knew when and where to go to catch fish on the high seas and in the depths as a result of the information furnished by their research men. They knew the best and most economical methods of catching, processing, and marketing their products—fields of research in which we have hardly made a beginning. One example is sufficient to show the difference between their methods and ours.

They had an albacore fishery of considerable size along the shores of the home islands in which the fishermen ventured as far as 100 miles from shore. We had a similar fishery for albacore along our west coast, which was similarly conducted close to shore. But the Japanese sent research vessels out into the Pacific, and by the time the war came along their fishery for albacore extended 2,000 miles straight out into the Pacific to the longitude of Midway Island. This high-seas fishery had come to overshadow the shore fishery in the bulk of its production. They were also catching albacore regularly in 1,200 feet of water.

We are still fishing albacore within less

Photo by Ray Krantz. From the Pacific Fisherman

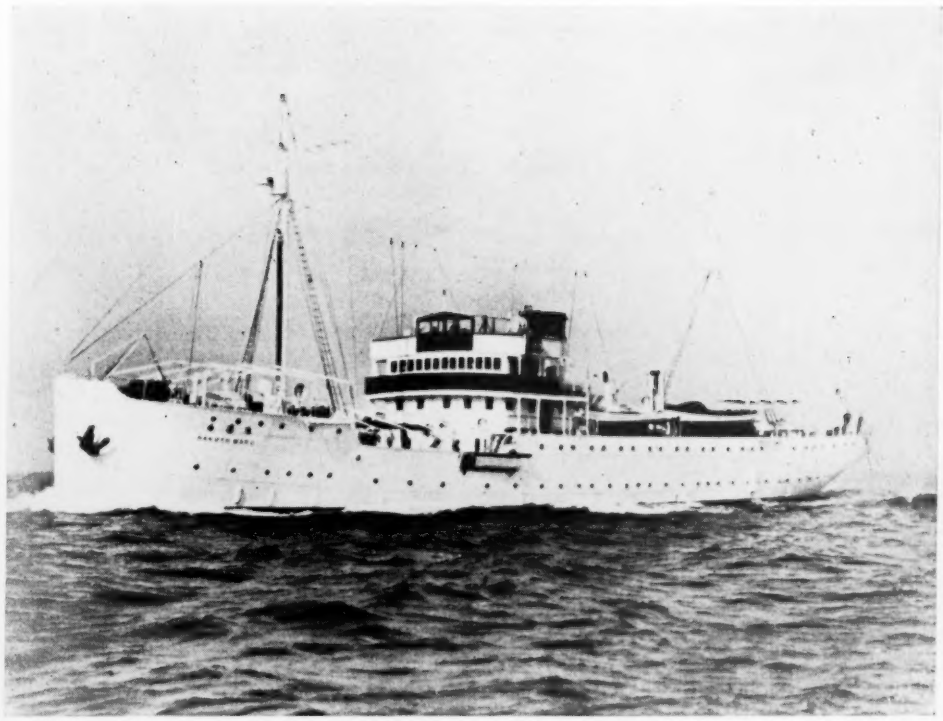
U. S. FISH AND WILDLIFE SERVICE VESSEL *HERON*

than 100 miles of our coast, and no attempt has been made to explore the high seas or the depths off our coast for the schools of albacore that the Japanese found off their shores.

We are still held in the grip of the theory that there can be no major quantities of harvestable produce in the middle of the ocean or in the depths because there is not enough food there to support large schools of fish. The Japanese, apparently not knowing about this lack of food, went right out and started harvesting.

What has been done by the Japanese can be done by us, but we shall have to put the same emphasis on high-seas research that they did. During this 30-year period in which the Japanese production of food from the sea has seen such a blossoming and in which they have so tremendously expanded their scientific research effort to find out about the possible productiveness of the sea, the United States Government has not had a single fisheries research vessel in the Pacific, save for one small boat in Alaska which was not large enough to get out from the shelter of the inland passage.

Our competent oceanic expeditions have been limited to *one*, which investigated the crab-productive capacity of the Bering Sea (after similar Russian and Japanese expeditions had covered the same ground). This lasted for only 2 years, but it brought back enough information about other fisheries besides crab to far more than pay the costs of the work. The only reason it was undertaken was that the Japanese had found methods to capture crab in our own waters, process them in packages attractive to the American housewife, and sell them in this country at a lower price than our own producers could meet.



From Dr. J. L. Kask

PREWAR JAPANESE FISHERIES INSTITUTE VESSEL *HAKUYŌ MARU*

We can learn the secrets of the production of the sea and turn them to our own benefit. The land area of the world is small, and one day there will come a time when it will not produce the food that its people require. The Pacific Ocean occupies about 10 percent more area than all the land in the world put together, and, while the land will produce only in its upper inches, the sea will yield produce in great variety to a depth of more than 1,000 feet.

In the tropical Pacific we have won an empire of tremendous size. It is an empire of great riches, where the land is as nothing and the sea is everything—an empire in which the native people are small in numbers and restricted to small points in its vastnesses; an empire which no other nation save the Japanese covets and which no other nation save theirs and ours can cultivate and make produce.

Off our own shores lie riches in as great quantity. Once our countrymen lived from the sea; then we turned inward to cultivate the land. Now the land is rapidly being filled, and it is time that we again turn our faces to the sea, where there is space for our future development.

LONGEVITY AND CASUALTIES AMONG NATURALISTS IN TROPICAL AMERICA*

By CARLOS E. CHARDON

Instituto Agrícola Nacional, Republica Dominicana

THE proverbial insalubrity of tropical climates, a subject about which volumes have been written and which the average American takes for granted, does not appear to be borne out by a study of the lives of the naturalists who have wandered through the jungles of the Amazon and the Río Negro, the great llanos of Venezuela, or the *pantanals* of Paraguay in search of natural-history specimens. Many of these devoted soldiers of the great army of science have, in spite of years of hardships and vicissitudes, reached old age, and examples of octogenarians are not uncommon. Others, by far a small minority, have paid dearly for the adventure and have passed down in history as the martyrs of a noble cause.

The longevity of four of the early chroniclers whose writings included descriptions of the flora and fauna of America is most striking. First among them was Gonzalo Fernández de Oviedo (1478–1557), a Spanish soldier and a friend of the sons of Christopher Columbus. He came to America in 1514 and made 4 subsequent trips, covering Hispaniola, the Darien, and the Spanish Main. He was the author of the *Historia General y Natural de las Indias*, the first portion of which was published in Seville in 1535 and reprinted in Madrid (3 vols., 1852). The book is remarkable for the observations of Oviedo on the flora and fauna of the New World. He was evidently "a born naturalist," as Paul C. Standley calls him. He died in Valladolid, Spain, at the age of seventy-nine.

* This study was partly made possible through a grant from the John Simon Guggenheim Memorial Foundation (1945–1946).

Next is the priest Bartolomé de las Casas (1474–1566), called the Apostle of the Indians and Archbishop of Chiapas. He crossed the ocean 14 times, lived in Hispaniola, Cuba, Mexico, and other colonies. It is remarkable that he started writing his famous *Historia General de Indias* (not published until 1875–1876) when he was seventy-eight years old and finished it at eighty-seven. He died in Spain at the age of ninety-two.

The Jesuit José Acosta (1534–1600) came to America in 1571 and remained until 1587. During this time he covered most of what was then known in the New World. He was the author of the *Historia Natural y Moral de Indias* (Seville, 1590), which was translated into all the learned languages. He had, above all, the right sense of proportion, and his grasp of the universe as an integral unit led Alexander Humboldt to consider him as "the first world cosmographer." He traveled also in Italy and died in Spain at the age of sixty-six, the youngest of the four.

Garcilaso de la Vega (1539–1617) was born in Cuzco, Peru, the son of a captain in Pizarro's army and an Indian woman of noble birth. That is why he is called "Garcilaso, the Inca." He led an interesting life: he spent his youth in Cuzco, later went to Spain to become a soldier, and finally entered the church. He gathered a wealth of information about the Inca Empire from his mother and other relatives and was author of the celebrated *Comentarios Reales* (Lisbon, 1609), which contains valuable information on natural history. His last years were spent in worship, and he died a priest at seventy-nine years of age.

The average life of these four is roughly 77

years and 6 months, and it is interesting to note that none of the subsequent groups did as well as these old-timers.

HEALTH conditions in America during the seventeenth and eighteenth centuries continued to be about the same as in the preceding century. Nevertheless, the life span of naturalists continued far above expectations. The story of a dozen of them, all high-ranking, follows:

The first, George Marcgrave (1610–1648), is the exception to the rule. He was born in Saxony and received a thorough scientific training in German universities. He went to Holland and sailed for Brazil in 1638 as part of the expedition of the Count of Nassau-Siegen. He was the author of *Historia Naturalis Brasiliae* (Amsterdam, 1640), which marks the beginning of scientific work in Brazil. He was later sent to Africa and died of fever in the colony of Angola at the age of thirty-eight.

The priest Charles Plumier (1646–1704) was the first French naturalist to visit this side of the Atlantic. He made 3 trips to the West Indies and traveled extensively in Hispaniola and most of the Lesser Antilles at a time when health conditions were frightful, according to Labat. He was the author of *Description des Plantes de l'Amérique* (1693) and several other books on botany, all very remarkable for the time. At fifty-eight he died of pleurisy in Cadiz, Spain, just as he was getting ready to sail on a fourth expedition to Peru to study Cinchona trees.

Sir Hans Sloane (1660–1753) came to Jamaica as the physician of the Duke of Albemarle. He collected many specimens of plants and animals and wrote *A Voyage to the Islands of Madeira, Barbados, Nieves, S. Christophers and Jamaica, etc.* (2 vols., 1707–1725). Sloane went back to England and became a great scientist, succeeding Newton as President of the Royal Society. He died at the age of ninety-three.

THE "INCA" (1539–1617)
FIRST AMERICAN-BORN NATURALIST; AUTHOR OF *Comentarios Reales* (LISBON, 1609).

Another French priest, Louis Feuillée (1660–1732), was a mathematician, physicist, and naturalist, a combination which cannot be duplicated in our modern times. He visited Chile and Peru during 1707–1713 and was the author of *Journal des Observations physiques, mathématiques et botaniques* (2 vols., Paris, 1714). He died in France at seventy-two years of age.

Charles Marie de la Condamine (1701–1774) was a mathematician and naturalist sent by the French Academy to measure an arc of the meridian in the region of Quito, Ecuador, to determine the correct shape of the earth. He spent 10 years doing this and then returned to Europe by way of the Amazon River. He published *Relation abregée d'une voyage fait dans l'interieur de l'Amérique meridionale* (Paris, 1745). He died in Paris, as a result of a surgical operation, at seventy-three.

NICHOLAS JOSEPH DE JACQUIN

BORN 1727, DIED 1817. GREAT BOTANICAL EXPLORER OF THE WEST INDIES AND THE SPANISH MAIN.

De la Condamine was accompanied by Joseph de Jussieu (1704–1779) as botanist of the expedition. He remained in South America for 35 years, traveling in the difficult Andean regions of Ecuador, Peru, and Bolivia, during which time he built an enormous herbarium. But fate was very cruel to de Jussieu: just as he was sailing for Europe his entire collection was stolen. Poor Joseph, brother of Antoine and Bernard Jussieu, arrived in France broken-hearted and insane, but still he managed to live until 1779; age, seventy-five years.

Jean Baptiste Christophe Aublet (1720–1778) was the French botanist and explorer who first visited the Far East. He later came to Cayenne to collect plants in 1762 and spent several years in the interior, where conditions even at the present time are most trying. He was the author of *Histoire des Plantes de la Guiane Françoise* (4 vols., Paris, 1775), a botanical classic of the eighteenth century. He died in Paris at the age of fifty-eight.

Patrick Brown (1720–1790) was born in Woodstock, Ireland. Succeeding Sloane in Jamaica, he spent several years in the colony as a physician and visited almost every parish. He was author of *The Civil and Natural History of Jamaica* (London, 1756). At seventy he died in England.

Baron Nicholas Joseph de Jacquin (1727–1817) was a native of Leiden and also a great botanical explorer. During a period of 7 years he explored most of the Lesser Antilles, Hispaniola, Cuba, and a portion of Venezuela. He was the author of *Selectarum systematica stirpium americanarum historia* (Vienna, 1763). Jacquin died a professor in the University of Vienna at the age of ninety.

José Celestino Mutis (1732–1808), a Spanish priest and the director of the Expedición Botánica established by Charles III of Spain, resided 48 years in Colombia, many of which were spent in Mariquita, a mosquito-infected region on the upper Magdalena River. Mutis was a friend of Alexander Humboldt. He prepared an enormous work called *La Flora de Bogotá*, with thousands of colored plates, which unfortunately was never published. He died at the age of seventy-six.

Louis Claude Marie Richard (1754–1821), French botanist and explorer who visited the Lesser Antilles, Cayenne, and a portion of Brazil, died in France when he was sixty-seven years old.

Félix de Azara (1743–1821), a Spanish military engineer and one of the pioneers in the study of zoology in South America, spent 17 years in the interior of Paraguay, living almost alone, except for a few Indians, in the midst of the jungle. He established the boundary line between the Spanish and Portuguese colonies. Don Félix was not a trained naturalist, but he was an exceptionally good observer. He was the author of *Apuntamientos para la historia natural de los cuadrupedos del Paraguay y del Rio de la Plata* (1802) and a

similar volume on birds (1805). He must have been a great character. He died at the ripe old age of seventy-nine in his native land, Spain.

The average age of the twelve naturalists is roughly seventy years and nine months. All of them, save one, José Celestino Mutis, went back to Europe.

THE eventful trip of Alexander Humboldt and Aimé Bonpland to America (1799–1804) marks a distinct epoch in the history of scientific exploration. Humboldt's *Voyage aux regions equinoxiales* had a profound influence on Darwin and scores of others who later came to America to explore its immense natural treasures.

Alexander Humboldt (1769–1859) was born in Berlin and spent his youth in intensive study and travel in Europe, getting ready for a great overseas voyage. In his early youth he was greatly influenced by his close and intimate friend George Foster (1754–1794), who accompanied Captain Cook on his second voyage of circumnavigation of the globe and was the author of a book called *A Voyage Around the World* (London, 1777). Humboldt was a great admirer of Foster's sister, but she married another, and he remained a bachelor, though there is some indication that he had other romances, judging from a slip he made in the introduction to *Aspects of Nature* (1808). Apparently his failures in love never depressed him; on the contrary, they seemed to act as a great stimulus to his other activities. He was described, from Caracas to Mexico City, as being very jovial and resourceful—traits of Latin temperament which made him immensely popular in social circles.

He and his friend Bonpland sailed from Corunna, Spain, and landed in Cumaná, Venezuela, on July 16, 1799. Their itinerary covered vast territories of little-explored lands, some of them the worst on the continent from the standpoint of health conditions. They covered the region of Cumaná and Caracas, visited Valencia Lake and Maracay, then turned south toward the Apure through the llanos of Calabozo; later they went up the Orinoco and explored the Casiquiare, proving that this river connected the Orinoco with the Río Negro. They sailed down the great river as far as Angostura and crossed again to Nueva Barcelona; from there they took a sailboat that brought them to Cuba. After a few months in Cuba, they sailed south, landing in Cartagena, and went up the Magdalena River to visit Bogotá and see Mutis. Then they went south again as far as Quito, Cajamarca, Trujillo, and Lima. From Callao, Humboldt sailed to Guayaquil and from there to Acapulco, Mexico. He visited Mexico City, its mines, its volcanoes, and Indian antiquities, sailed from Veracruz to the United States, and from there to Bordeaux, France.

DON FÉLIX DE AZARA (1743–1821)
SPANISH MILITARY ENGINEER AND GREAT NATURALIST OF PARAGUAY AND THE RIVER PLATE.

Humboldt's and Bonpland's trip to America lasted five years. I have followed

their trail along the llanos and the Apure River and have often wondered how they ever got through. Yet they managed to do it with but two incidents: Bonpland had an attack of fever (probably malaria) on the upper Orinoco and later suffered from the same malady while going up the Magdalena River in Colombia.

The rest of Humboldt's life in Europe is well known. He became one of the world's celebrities and died a very old man in Berlin.

Aimé Bonpland (1773–1858), whose correct name was Aimé Jacques Alexandre Gougaud, was born in La Rochelle. His early love for plants so captivated his mind that his father used to call him *Bon-plant*, and he has passed on to posterity as Bonpland. He was a physician by profession but could not resist becoming a botanist. After his return to Europe from America, he was appointed superintendent of the Jardin des Plantes and became a close friend of the Empress Josephine. After she divorced Napoleon I, Bonpland was one of the few who remained loyal to her, and he was present at her deathbed. Josephine's death was a severe blow to Bonpland, and he decided to return to America. After a short stay in Argentina, he selected Paraguay, the most remote country of South America, for his future studies on plant life. All the efforts of Humboldt and the kings of France and Prussia to induce him to return to Europe failed to impress him. He spent 10 years in jail under dictator Francia, of Paraguay. Later he bought a small *rancho*, married, and had several children.

Little is known about Bonpland's life in Paraguay, except that he came once to Montevideo and presided at a banquet to commemorate the fall of Sebastopol in the Crimean War. He died an old man, in a miserable hut, abandoned by his *mestizo* wife, far away from his friends and civilization.

FELIPE POEY (1799–1891)

BORN IN HAVANA AND TRAINED IN FRANCE, HE WAS THE GREATEST OF THE CUBAN NATURALISTS.

NINETEENTH- and twentieth-century naturalists have been divided into geographical groups, and the average longevity for each group has been separately determined, as shown below:

The West Indies, including Trinidad: Total, 36; average age, 65 years.
Mexico and Central America: Total, 29; average age, 65 years.
Venezuela, the Guianas, and Colombia: Total, 20; average age, 72 years.
Ecuador, Peru, and Bolivia: Total, 16; average age, 68.5 years.
Brazil and Paraguay: Total, 34; average age, 70 years.
Grand total, 135; general average, 67.7 years.

Of these naturalists, the outstanding examples of those living 80 years or more are as follows:

John Donnell Smith (1829–1928) was born in Baltimore, Md. He became an artillery captain in the Confederate Army, graduated in law from Yale, and acquired relative wealth in business. He retired in

1880 to become a botanical explorer, a profession he had long cherished. He collected plants in many states of the Union but later devoted himself to the study of the flora of Central America. He made many expeditions to Guatemala and Costa Rica, gathered an immense herbarium of 100,000 specimens, and published many papers describing hundreds of new species of plants. Smith had the poise of a distinguished Southern gentleman. Just six months before reaching the century mark he died of old age.

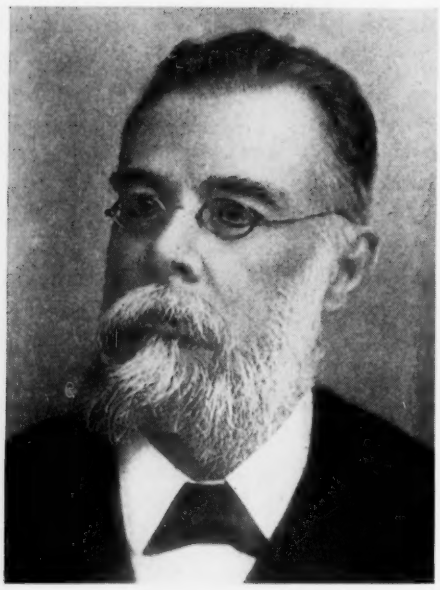
Felipe Poey (1799–1891) was born in Havana and trained in Paris. The founder of natural-history studies in Cuba, he became a distinguished zoologist, especially in the fields of icthyology and entomology. He was the author of *Memorias de la Historia Natural de la Isla de Cuba* (2 vols., 1851–1861) and a dozen other works. While young he contracted a disease in France which paralyzed one of his limbs. Poey lived the rest of his life in Cuba, which at that time was no paradise for a white man; yet he lived to be ninety-two.

Hermann Karsten (1817–1908) was born at Stralsund, in the Baltic. He started his training under an apothecary in his home town but later went to Berlin, where he studied medicine and natural science. In 1844 he went to Puerto Cabello, Venezuela, where yellow fever was endemic, and explored north-central Venezuela until 1847, at which time he returned to Europe. Karsten came back to America the following year, collected extensively in the Andean region, and later went to Unagua, Barcelona, Cumaná, the Gulf of Cariaco, and Cumanacoa, a region still heavily infected with malaria. Later he visited Maracaibo and Colombia and published *Florae Columbiae* (2 vols., Berlin, 1858–1869), splendidly illustrated. He died at the age of ninety-one in Germany.

Alfred Russell Wallace (1823–1913) was born in Monmouthshire, England, and was a great friend of Charles Darwin. He traveled extensively in the Amazon and Río Negro country during the period from 1848 to 1853. His brother Herbert came to America to help him but died in Pará of yellow fever. On Wallace's return trip to England, his ship caught fire. Just as the water and provisions had been exhausted the small boat in which he and some other survivors escaped was rescued near the Bahamas by the *Jordesen*, which was sailing from Havana to England. He later explored the Malay Archipelago and while stricken with fever conceived, independently of Darwin, the theory of evolution. He died of old age in England at ninety.

Theodor Peckolt (1822–1912) was born in Pechern, Silesia. He was a physician and pharmacist who came to Brazil in 1848. Peckolt traveled extensively on horseback, curing the sick and refusing remuneration. He welcomed natural-history specimens, however, and information on medicinal plants. After 20 years of exhausting work (traveling always on horseback) he settled in Rio de Janeiro and established a drugstore, but his interest in medical botany continued. He was the author of a monumental work called *Historia das plantas medicinaes e uteis do Brasil* (8 vols., Rio de Janeiro, 1888–1914). After three-quarters of a century of living in the tropics, he died of old age in Rio at ninety.

Hermann von Ihering (1850–1930) was born in Giessen, Silesia. He specialized on the Mollusca in Erlangen. In 1880 he married and came to Brazil on his honeymoon. During the next 12 years he published 77 scientific papers on various phases of zoology and anthropology. Ihering became the Director of the Museu do São Paulo, which is now called the Museu Paulista. He was also the founder of the Archelenis theory, which conceives the early continent of South America to have been made up of two separate units, "Archiguiana" and "Archiplata," separated by a



HERMANN VON IHERING

BORN 1850, DIED 1930. HE WAS A DISTINGUISHED GERMAN ZOOLOGIST AND FOR MANY YEARS WAS DIRECTOR OF THE MUSEUM OF SÃO PAULO, BRAZIL.

vast ocean now occupied by the Amazon Valley, with the Andes coming later in the Tertiary, connecting both and constituting the continent's present backbone. During the first World War he had to leave Brazil and died in Germany at the age of eighty.

William H. Edwards (1822–1909), a descendant of Jonathan Edwards, was born in a small village in the Catskill Mountains. He studied law at Williams College and visited the Amazon in 1842, from Pará to Manaos. His book *A Voyage up the River Amazon* (London, 1847) had great influence in inducing Wallace, Bates, and Spruce to come to America. He later became a successful businessman in the United States: built railroads, exploited coal mines, etc. Yet he always found time to study Lepidoptera and published *Butterflies of North America* (First edition, 18 parts, 1868–1896). He lived to be eighty-seven.

Eugène L. Simon (1838–1924) was born in Paris and became one of the greatest arachnologists of all time. He traveled extensively in the following countries: Sicily, Spain, Corsica, Morocco, Algeria and Tunisia, Suez and Aden, the Philippines, Ceylon, and South Africa. He visited Venezuela during the years 1887–1888 and collected spiders, insects, and hummingbirds. He was the author of *Histoire Naturelle des Araignées* (8 fasc., Paris, 1892–1903). Simon had a wonderful memory and remembered the names of thousands of spiders, the places he collected them, and the exact literature concerning each species. He died in Paris at the age of eighty-six.

Jean Baptiste Boussingault (1802–1887) was born in Paris and became one of the founders of agricultural chemistry. Boussingault went to South America at the request of the liberator Simón Bolivar to organize a scientific institute in Santafé de Bogotá. Landing in La Guaira when he was twenty, he traveled extensively in Venezuela, Colombia, and Ecuador, under the most trying circumstances, during the years 1822–1832. Boussingault served in Bolivar's army as colonel of the staff. His *Memoires*, limited to 300 copies, was published 5 years after his death. His account of his visit to the out-of-the-way Chocó region is very exciting, for he said that in order to evade the attack of the ugly *carate* disease he sucked the milk of a young negro mother. He was later President of the French Academy and died peacefully at the age of eighty-five.

Henry Hurd Rusby (1855–1940) was Dean of the College of Pharmacy of Columbia University and a great botanical explorer of Bolivia. His book *Jungle Memories* (New York, 1933) is one of the best on travel ever published. His first visit to Bolivia was in 1887–1888, when he crossed the tableland and came down the rivers Beni, Madeira, and Amazon to Pará. His second visit was very amusing and was known as the Mulford Biological Expedi-

tion, 1921. It was a most carefully planned expedition: proper food, drugs, and medicines and vaccines against every possible ailment were provided, and every member of the expedition received detailed instructions as to what to do at the proper time. The *Entomological News* for that year published periodical releases on the progress of the expedition, but something unexpected happened. Just as he was in the midst of the jungle, Rusby developed a very severe toothache caused by an infected tooth. This triviality had not been provided for, and the director had to return to New York for treatment and operation. His companions carried on in a most gallant way: They were William M. Mann, entomologist, who is now the Director of the National Zoological Park, and Orlando E. White, botanist, now Director of the Experiment Station at Boyce, Va. Rusby soon recovered, continued his botanical work, which was both fruitful and useful, and died peacefully at eighty-five.

The other octogenarians are: William Schaus, eighty-four; Antoine Duss, eighty-four; Charles Waterton, of Guiana fame, eighty-three; Manuel Villada (Mexican), eighty-three; John Burchell, eighty-one; Frank M. Chapman, eighty-one; Everard im Thurn, eighty; Edward Palmer, eighty; Nicholas Funck, eighty; and Richard Schomburgk, eighty.

In a study of longevity among the naturalists in the American tropics it will be interesting now to make reference to the number of casualties. For the sake of convenience these may be divided into 3 categories: (a) those due to disease; (b) accidental death; and (c) suicide. Out of 135 naturalists studied, 4 belong in the first category, 3 in the second, and 2 in the third. A paragraph about each of these naturalists may be of interest.

James Orton (1830–1877) was born in Seneca Falls, N. Y., and later became Professor of Natural Science at Rochester University. He was sent by the Smithsonian Institution on an expedition through the Andes of Ecuador in 1867 and came back by way of the Amazon River. In 1869 he was appointed professor at Vassar College and published a book, *The Andes and the Amazon* (London, 1870), which was very popular. In 1873 he made another expedition to South America, reversing his earlier route. A third expedition was started in 1876 to explore the Beni River, a tributary of the Madeira, in northern Bolivia. The escort sent by the Bolivian government abandoned him and his companions, and they had to make a terrible journey of 600 miles with little food until they reached La Paz. Orton was suffering from exhaustion and overexposure but continued his job in a vain effort to reach the coast. When he was crossing Lake Titicaca he died in a totora boat and was buried on one of the islands of the lake.

William Ashbrook Kellerman (1850–1908), a mycologist, was born in Ashville, Ohio. He studied at Cornell and later visited Europe and received his doctorate in Zurich. After teaching in the agricultural colleges of Wisconsin and Kansas, he went to Ohio State University, where he built a large herbarium and started editing the *Journal of Mycology* (the first periodical of its kind in America). He made 3 trips to Guatemala for the purpose of collecting fungi, especially plant rusts. On a fourth trip he was accompanied by 3 of his students. At the end of this trip, after the ascent of a high mountain, Los Amates, he complained of feeling very weak, was attacked by fever, and died during the night of March 8, 1908.

Of A. H. Fassl-Teplitz, a German explorer, little is known except that he was a first-class collector of butterflies. He made collections in Colombia during 1907–1911. In 1912 he made another trip to South America, landed in Buenos Aires, and

climbed the high Andes of Bolivia, publishing an account of this trip in Darmstadt (1920). His collections were enormous, prepared with great care, and were of exquisite beauty. He returned to the jungle and died in Manaos, in 1922, of malaria.

Erick Leonard Ekman (1885–1930) was a Swedish botanist and explorer. He came to Cuba in 1914, on a Regnell Scholarship, to collect plants for Professor Urban's *Symbolae Antillanae*. He explored the island extensively from seacoast to mountaintop. Later he explored Haiti and the Dominican Republic, where he did a very thorough job. He was an eccentric man, living on almost nothing in the way of clothing and food. He always slept in the open in all kinds of weather. Under these conditions he could not live long in any climate and he died in Santiago de los Caballeros, Dominican Republic, of a high fever, which easily overcame his already weakened body.

Carlos Bertero (1789–1831) was an Italian botanical explorer. He visited Guadeloupe, St. Thomas, Puerto Rico, Haiti, Santo Domingo, and part of New Granada (Colombia). He sent large collections of plants to Europe; in 1830 he continued his botanical work in Chile and explored the Juan Fernández Islands (supposed to be *Robinson Crusoe's* setting). A year later he decided to visit Tahiti and other islands of the South Sea, but his vessel was sunk with all men on board.

Fr. Sellow (1789–1831) was a German collector who accompanied the Prince of Wied to Brazil in 1815. When the prince returned to Europe, Sellow continued his work in Brazil and made large collections of plants, especially in the San Francisco River Valley and the interior of Minas Geraes. He was drowned while bathing *à la nude* in the Río Duce ("Sweet River").

Ferdinand Nevermann (1881–1938) was born in Hamburg, Germany, and was a general biologist. Nevermann went to Costa Rica in 1909 to engage in farming but devoted himself, beginning in 1921, to the study of Coleoptera and insects in general. He sent large collections of insects to Europe and published several papers in Costa Rica. His was the toughest luck of all. On the night of June 30, 1938, while he was collecting ants in the dark, a hunter mistook his silhouette for that of a puma and shot him.

William John Burchell (1782–1863) was born in Foulsham, England. Burchell was sent in 1805 by the West India Company to the far-off island of St. Helena, where he stayed 5 years as a botanist. He had left his sweetheart in England and continued corresponding with her until he persuaded her to come and marry him. The trip was a long one, and the captain of the vessel was so intrigued by her beauty that he married her. This unexpected breach of promise greatly upset Burchell, who decided to leave for the Cape of Good Hope, where he made large zoological and botanical collections. He went to Brazil in 1825 to continue his explorations and returned to England in 1830. He lived in Foulsham for 3 decades in the midst of his collections and notes, not daring to show them to anyone for fear they might be stolen. In 1863 he committed suicide at the age of eighty-one.

Karl Ferdinand Appun (1820–1872) was a German ornithological collector who traveled extensively in Venezuela. He published a book of travel entitled *Unter den Tropen* (2 vols., Jena, 1871), which is very readable and well illustrated. He was an eccentric man, traveled very seldom in company, and had a persecution complex, so he carried always a little bottle of concentrated sulfuric acid in his pocket. One day in the midst of a Guiana forest he committed suicide by pouring the acid over his face.

It is difficult to explain such a low mortality through disease—less than 3 percent—and such longevity—an average of sixty-eight years and four months—among the

naturalists in the tropics of America. An attempt, however, may be made, considering only one of the factors involved, the neuropsychic. MacCartney's work on tropical neuropsychiatry, as reviewed by Ellis H. Hudson ("Tropical Medicine: Its Scope and Present Status." SM, January 1944, p. 47), throws some light on this dilemma:

> Strange environment, foreign language and customs, unaccustomed food, intense light and heat, tropical rains, ubiquitous and exasperating insects, all are factors which, combined with the monotony of existence, the lack of amusements, and restrictions of companionship, lead to neuroses and depressions. Least successful are the egocentric and rigid personalities lacking poise or given to idiosyncrasies and complexes. Most successful are the sanguine, the adaptable and resourceful, and the intellectually curious.

Some statements in the above paragraph are interesting and offer a possible clue. For the naturalist in the jungle, there can be no "monotony of existence, lack of amusements, or restrictions of companionship, which lead to neuroses and depressions." Burchell, the brokenhearted, and Appun, the psychopathic, are the exceptions, and both committed suicide. But no one can imagine the great Martius, or the celebrated Humboldt, or the indefatigable Natterer, or Frank Chapman, or Ducke, or a hundred others ever suffering from psychoneurosis or homesickness. Several years ago, I attended in Caracas a luncheon given by Mr. William H. Phelps to a group of scientists and I shall never forget what Dr.

CARLOS BERTERO (1789–1831)
AN ITALIAN BOTANICAL EXPLORER OF THE WEST INDIES AND CHILE. HE WAS LOST AT SEA.

Alexander Wetmore said in referring to his vicissitudes in the field: "I would not change my field experience for all the treasures of India."

Wetmore's reaction was the sincere expression of a true scientist and a great explorer. He was indeed right. To the naturalists in the field there are no worries as the rest of us think of them. The naturalists, as a group, are all "adaptable, resourceful, and intellectually curious." No wonder, in their minds, there is no room for hysteria.

A 1947 ECLIPSE EXPEDITION

SPONSORED BY THE NATIONAL GEOGRAPHIC SOCIETY AND THE U. S. ARMY AIR FORCES

By LYMAN J. BRIGGS

Research Committee, National Geographic Society

ON MAY 20, 1947, the total eclipse of the sun will be visible in east-central Brazil between 9:30 A.M. and 10:00 A.M. At that time the altitude of the sun will be in excess of 40°, affording an excellent angle for observation.

A group of scientists of the United States, under the sponsorship of the National Geographic Society and the U. S. Army Air Forces, has for the past year been planning an expedition to some place in the path of totality in South America to conduct a full program of studies of eclipse phenomena. Recently a reconnaissance party flew to Brazil, established cooperative relations with government officials, and selected a site near the small town of Bocayuva in the state of Minas Geraes. It has been located by preliminary observations at Latitude 17° 15′ S., Longitude 43° 42′ W., and is about 400 miles north of Rio de Janeiro on a dry plateau at an elevation of 2,300 feet. On the level area chosen, the Army Air Forces will set up a base camp for housing and feeding the civilian scientists and their own officers and men who will take part in the expedition. The important instruments and much of the equipment will be flown to the camp.

In addition to the representatives of the National Geographic Society and the Army Air Forces, the party of observers will be made up of physicists, astronomers, radio engineers, and other specialists from the National Bureau of Standards, Georgetown Observatory, the Naval Research Laboratory, Yerkes Observatory, and Lick Observatory.

Dr. C. C. Kiess, of the National Bureau of Standards, assisted by Dr. Harold Weaver, of Lick Observatory, will attack special problems in astronomy and spectroscopy. Using two high-power spectrographs provided with two of the best concave gratings ever ruled, they will make high-dispersion spectrograms of the flash spectrum of the sun and of the solar corona. The gratings are mounted so that they can cover the entire photographable range of the solar spectrum from 3,000 angstrom units in the ultraviolet to 10,000 in the infrared. When nine-tenths of the sun's disk is covered Dr. Kiess will photograph in rapid succession the diminishing crescent. These exposures will provide data for evaluating the absorption coefficient of the sun's atmosphere. Such information is important in arriving at a better understanding of the radiative processes that go on in the sun's atmosphere and in radio broadcasting on the earth. In recording solar images Dr. Kiess and Dr. Weaver will make a special effort to obtain the fainter lines in the corona.

Dr. I. C. Gardner and Mr. W. L. Scott, of the Bureau of Standards, using an astrographic camera of nine-inch aperture, will photograph the corona both in black and white and in color. They will also study the polarization of coronal light with two cameras of 61-inch focal length. All three of these cameras were designed by Dr. Gardner. Because of the great range of brightness of the corona, variable density filters will be used in the large camera to black out the center part of the picture in order that an evenly exposed image of the faint portion of the corona can be made.

One of the smaller cameras will be used to determine how far out an image of the corona can be obtained. The second small camera will be operated with a polaroid filter so that successive pictures will show the state of the polarization of the corona.

Engineers from the Radio Division of the Bureau of Standards will measure the changes which take place in the ionized layers of the earth's atmosphere as the shadow approaches and passes on. Since the Division operates a series of stations throughout the world these stations will make observations at the same time that studies are being made at the eclipse camp. The observations will be used to obtain the distribution and variation of the ion density and also to provide data for computing the recombination coefficient. Information about the lower layer of the ionosphere is already fairly complete, but much less is known about the upper layer. The work, therefore, will be particularly concentrated on the latter level. A special effort will be made also to find whether there are noticeable ionization effects when particular parts of the corona are eclipsed.

Professor George Van Biesbroeck, of Yerkes Observatory, will undertake accurate measurements of the Einstein shift in the apparent positions of stars whose rays pass close to the sun in reaching the earth and for that purpose is now constructing a special telescope with a focal length of about 20 feet. Since the coronal light contains a considerable amount of green, it is desirable to absorb these and shorter wave lengths by means of a filter because this coronal radiation tends to mask the images of faint stars. By using only the red end of the spectrum, it is possible to record the images of stars close to the sun's limb where the Einstein shift is the greatest. The recent development of photographic emulsions which are highly sensitive to red light makes this practicable. But in order to utilize this advantage to the fullest extent, the telescopic objective itself must be so designed as to give its sharpest definition with red light, in contrast with the general practice of constructing telescopic objectives to focus most sharply with blue light. Accordingly, a new 5-inch objective has been designed and constructed for this work at the Yerkes Observatory, corrected to give its sharpest definition with red rays. The optical glass used in this objective was made in the glass plant at the National Bureau of Standards.

Professor Van Biesbroeck is also equipping his telescope so as to superimpose upon the star field coming from around the sun a second star field that will come in at 90° to the axis of the telescope and from a direction at the same altitude as the eclipse field. It is planned to leave the instrument in place for about five months, the time necessary to bring into the evening sky the star field which will be behind the sun during the eclipse. In order to get as much information as possible about the shifts as they are affected by the refraction of the atmosphere, it is proposed to make an extended survey of temperature conditions in the atmosphere from the ground up to 20,000 feet or more at the time of the eclipse and on preceding days. This work will be done by the Army Air Forces by means of instruments carried aloft in airplanes and sent up by sounding balloons. The Army Air Forces will also provide equipment for receiving the accurate time signals needed by the civilian scientists.

Dr. Paul A. McNally and Dr. Francis J. Heyden, of Georgetown Observatory, will make a large number of photographs for the purpose of measuring with precision the time at which the moon makes its four "contacts" with the sun's disk to provide additional experimental data relating to the theory of lunar motion. They will use a special film of such fine granulation that it will give needle-sharp images, which facilitate measurement. To supplement

an existing set of very fine photographs of the northern Milky Way, Dr. McNally plans to spend several weeks before and after the eclipse seeking to obtain equally good photographs of the southern Milky Way.

Dr. E. O. Hulbert, of the U. S. Naval Research Laboratory, will measure the brightness of the zenith sky at various altitudes as the shadow of the moon moves over the observation point. As a part of this work he desires to determine the molecular density and temperature of the air as high as possible. If all conditions are favorable, he should be able to get measurements of the temperature up to 60–70 kilometers.

World-wide interest is being taken in the forthcoming eclipse. Scientists from Brazil, Argentina, Australia, New Zealand, Russia, Sweden, Finland, and Denmark have announced their intention of making observations at various points in Brazil. Of particular interest are the plans of the group from Sweden, Finland, and Denmark, who propose to utilize the eclipse in a new determination of the figure of the earth. The proposal originated with Professor Bonsdorff, of Helsinki. Briefly, it consists in measuring with great precision the times at which the second and third contacts occur as observed at two stations widely separated along the center line of the path of totality. For the forthcoming eclipse it is proposed to locate one station in Brazil and the other in West Africa. From these times of contact the length of the chord of the great circle passing through the stations can be computed. If the mean error in timing the contacts does not exceed ± 0.04 second, it is believed that the distance between the two stations may be determined with a mean error of ± 60 meters. The American expedition has volunteered to make available to this group the accurate time signals received from the Naval Observatory, the Bureau of Standards, and Greenwich.

Inasmuch as the time of contact actually extends over a period of about 6 seconds, owing to the irregularities of the surface of the moon, a question arises as to how a precision of ± 0.04 second may be secured. Two methods will probably be used. The first consists of a series of photographs made at very short intervals, the exact time being known at which each exposure was made. By comparing the photographs from the two stations, some event in common, such as the disappearance of a particular Baily bead, may be recognized. The second method consists of a similar series of spectrographic records, in which the spectral lines will be crossed by lines and bands produced by the mountains of the moon. Here particular events may again be recognized on the spectrograms of the two stations, especially with the aid of densitometer measurements.

ALL OF A KIND, CAPER TO CANTO

By W. J. HUMPHREYS

Cosmos Club, Washington, D. C.

EACH of the three great rhythmic arts—the Dance, Music, Poetry—merits large space in a treatise on physiology, for each is a manifestation of that interaction between nerve and muscle which, when rhythmically repeated, induces the pleasurable feeling of mental ease and physical well-being. The most primitive of these arts and the first any of us ever practices is the dance—rhythmic movements of the whole body or any portion or portions of it: the entire body in the whirl and glide of the waltz; legs and feet in the clog; hands and arms only in certain symbolic oriental dances; and even the abdomen alone, in that most difficult, perhaps, of all dances, the *danse de ventre*. But whatever the dance, simple or intricate, solitary or with one or more partners, its characteristic feature is rhythm. If the movements are irregular and chaotic, they are not a dance.

But why the dance? Nowadays some, like the ballet girl, dance for pay; some for show; some because it is a social custom; some because of the exhilaration it affords them; some to work up and display battle fury; some to express worship; and others for this or that whatnot else. All these dances, however, are according to forms set by rules or fixed by custom; not one is truly impulsive and spontaneous, such as the dance of a child before its father's knees on his return from a long absence or the capering and cavorting of his shepherd dog, both of which joyous displays and many others like them appear to be caused by strong emotion, for surely emotion does beget motion.

And in turn the motions of the dancer beget emotions in the minds of the observers, emotions that often cause them to feel the same movements of their own muscles and even induce them to join the dance. Just how these results are effected the biochemist and the laboratory psychologist each can tell us a little in terms that make sense; the armchair psychologist can tell us all about it in mystic words that leave our confusion as badly confounded as his own.

Nor to induce motion is it necessary that the emotion be ecstatic, or even joyous, for motion is a universal and spontaneous sign language of comprehensive range, from the friendly wink of the eye to the angry shake of the fist.

Why our movements tend automatically to be rhythmic, and therefore a dance of a sort, is not entirely clear, but partly, at least, it is because such motions produce less fatigue than does an equal rate of physical exertion irregularly expended. This is true because rhythmic movements are so automatic—so habit-like—that they require practically no mental attention, just muscular exertion, whereas irregular motions demand not only muscular effort but also such close attention as to produce mental fatigue as well, a sort of weariness that is worse than just being tired. Hence, physical work of any sort is least tiresome when done rhythmically. With even strides one can walk longer and farther than he can by going hippety-hop. Hence, too, the even pulls of the oarsman alone or in time with the rest of a crew; the regular strokes of the rock driller's sledge; the even swings of the farmer's scythe; the to-and-fro flip of the weaver's shuttle; and hundreds of other kinds of work, scarcely more than pleasantly tiring when

done rhythmically, are fatiguing beyond endurance when the necessary movements are so irregular as to demand constant close attention. This is one reason why the beginner at any one of many sorts of manual labor tires so quickly—he doesn't have the "swing" of it.

Similarly a series of sounds rhythmically repeated is far more pleasing to the ear than a succession of notes that go on and on without any semblance of order or repetition. We catch the swing of the thing and feel the mental ease it affords. At each note we sense what is coming next and experience no mental strain about it—even feeling, perhaps, simultaneous muscular reactions, however still we outwardly may seem to be. This is music, a succession of sounds rhythmically ordered. Whether any given piece of such music is liked by a particular individual depends upon his personal taste, which in turn depends largely upon what he has been accustomed to hearing. Chinese singing, which may put an American prima donna's teeth on edge, is loved by the millions who grew up with it; hers, which would give a mandarin the jimjams, we pay much to hear. The skirling bagpipe, however much disliked by most people, thrills the Scotchman beyond measure. The whining musette is far from pleasing to every ear, but Shriners proudly follow its lead as they go marching on; and so it is even with the tom-tom, whose insistent beat, annoying at first, finally will get you if you don't watch out.

In all these widely different sorts of music there is one quality, the least common denominator of all that is recognized by anyone as music, namely, rhythm. Of course there is much beside rhythm in most music, but rhythm, at least, it must have.

How, then, did music get started? That is another story and one of which we have neither history nor even tradition. Surely it first was vocal and possibly incident to rhythmic labor like, say, the swinging of a sledge hammer or the pulling of an oar. Here at the end of each heavy blow, or strong pull, the pent-up breath causes, as it suddenly escapes, an involuntary grunt that may be, and often is, in the form of a short explosive word or, when the work is not too strenuous, two or three words, the first being the start and the last the explosion end of the grunt, just as we often hear them today under such circumstances. Eventually this one- or two-word grunt was lengthened, as it now frequently is, especially by a slow worker, until it covered the whole interval from start to end of each strenuous effort, over and over again—vocal sounds of whatever easy sort set in rhythmic order. Vocal music this is, easy to imitate, with endless variations, even without the stimulus of regularly repeated physical exertion, though most likely and best sung, perhaps, when there is a recollection of such work in the singer's mind.

With this vocal start, instrumental music, however varied and elaborate, is but a sequel, and through it all the basic element is rhythm. And the same element, rhythm, pertains also to all real poetry, whatever the language. True, in much printed material that looks like poems it does not exist at all, but these are poems without poetry.

Caper to canto, then, the Dance, Music, Poetry, all is restful rhythm, producing a minimum of nervous fatigue for a given amount of muscular exertion, actual or fancied—actual in the dance, fancied in poetry. Each concerns the interplay between nerve and muscle and therefore is a subject of prime importance to the physiologist. The dance, music, and poetry all please us, but it is for the physiologist to tell us why they do.

A BIOPHYSICS SYMPOSIUM*

THE PARTICLE PHYSICS APPROACH TO BIOLOGY

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SINCE I know nothing of philosophy and very little of formal logic, my only service in a symposium of this sort is that of a specimen or exhibit. My role, as I see it, is to provide for discussion an exhibition of the typical naive biologist who thinks about the structures and functions of living things in terms of particles and tries to interpret these structures and functions in terms of the physical properties of the particles.

Particle physics contributes to biology in two ways. First, it contributes points of view. The biologist who uses the approach of particle physics thinks of the ultimate structures and functions of living things in terms of the physical particles, their aggregates, and their interactions with each other. We try to explain all the activities of organisms in terms of the activities of the ultimate particles. Second, particle physics provides powerful techniques for the investigation of biological structure and function. This, perhaps, is inevitable if one bases his biological concepts on the particle point of view.

Let us consider the manner in which our naive biologist regards the structure and activities of an organism in terms of the properties of the ultimate physical particles.

The ultimate particles currently well recognized in physics are the positron, the electron, the proton, and the neutron. So far as we now know, the positron is inclined to evanescence and rarely figures in biological phenomena; we shall therefore ignore it in this discussion. The remaining three particles, however, are directly or indirectly of extreme importance. The electron has extremely small mass and a negative electric charge. The proton has a positive electric charge equal in magnitude to that of the electron, and its mass is some 1,700 times as great as that of the electron. The neutron is electrically neutral and has a mass very close to that of the proton. All these particles have magnetic properties, and the electron and the proton, being electrically charged, give rise to electric fields even when at rest.

According to current atomic models, the nuclei of atoms consist of protons and neutrons and therefore have net positive charges. The relatively vast extranuclear volume of an atom is occupied by a cloud of electrons, each of which occupies an "orbit" which is mathematically describable. In an electrically neutral atom, the number of orbital electrons is equal to the number of protons in the nucleus; this number is known as the atomic number and identifies the chemical element to which the atom belongs. The number of neutrons in the nucleus, for a given number of protons, is somewhat variable but not indefinitely so, because too great variation leads to instability of the nucleus. This means that atoms of the same element may have different masses; the various kinds of atom of the same element are called isotopes. We shall return to these later.

Since the orbital electrons, and particu-

* This article and the two following ones by Drs. Burr and Margenau are based on papers presented at a symposium on The Philosophy of Biophysics at the annual meeting of the A.A.A.S., St. Louis, April 1946, in a joint session of Section L and the American Philosophical Association.

larly those in the outermost orbit, are the portions of the atom which are closest to neighboring atoms, these electrons are the particles whose intrinsic properties and arrangement chiefly determine the behavior of that atom toward its neighbors. This behavior is collectively what we know as chemistry, and if we are studying the behavior of an atom in an organism, we are studying biochemistry. Since the configuration of the orbital electrons is chiefly determined by their number and by the properties of the electrons themselves, and since the number of electrons in an atom is dependent upon the number of protons in the nucleus, we can see that chemistry, including biochemistry, is dependent upon the ultimate particles of which atoms are built.

Because of the configuration of the orbital electrons, each species of atom has a characteristic behavior toward other atoms, and in particular it may enter into atomic aggregates which we mostly call molecules. The number of species of possible aggregates is large, as might be expected, and in biochemistry we encounter an extremely large number of certain of these molecular species. These atomic aggregates, or molecules, have certain means of reacting with others, and these potentialities we consider to be determined by the properties of the constituent atoms, which in turn are determined by the properties of the ultimate subatomic particles.

Our naive biologist will go even further and visualize a microscopic biological cell as consisting of molecules, ions, and colloidal micelles (aggregates of atoms or molecules), all arranged, both spatially and temporally, in ways determined by the properties of the ultimate physical particles in the cell and in its environment. And he will go even further and regard a complex organism essentially as an orderly aggregate of cells and their products. This complex organism may have certain macroscopic properties, such as gross anatomy, symmetry, and gradients of various sorts, which can be studied very profitably without reference to particle physics, but the particle biologist regards all these properties as derivatives of particle properties.

We have just seen that the concepts of particle physics enable the biologist to think about biological phenomena in potentially very simple terms. This is one of the great advantages of taking the particle viewpoint. However, this advantage in viewpoint is at least matched, and possibly overshadowed, by the wealth of techniques which particle physics furnishes for attack on biological problems. For instance, consider the various devices made possible by taking advantage of the properties of the electron: the vacuum tube, the X-ray outfit, the electron microscope, and the electron diffraction apparatus. One could extend the list of particle techniques almost indefinitely, but, further to exemplify the naive biologist, I shall explain in somewhat greater detail some of the particle techniques with which I happen to be most familiar.

First, let us consider the application of ionizing particles in biology. These ionizing particles are either electrons or atomic nuclei of various sorts. These fast charged particles may be primary radiations, or they may be set in motion by means of other radiations such as X-rays, gamma-rays, and neutrons. Under certain conditions, very substantial beams, either of electrons or of light atomic nuclei, may be produced artificially by means of the so-called atom-smashers, such as cyclotrons, Van de Graaff generators, etc.

These ionizing particles have been used in basic and applied biology in various ways. For instance, for two decades, they have been used in genetics to produce mutations. Here the fast ionizing particle presumably produces some alteration in the

particle make-up of the gene or of some grosser structure such as a chromosome or chromatid. The geneticist usually is not very curious about the exact nature of the physical action of the fast particle upon the genetic make-up of the cell, but he has a very intense interest in the end result—namely, the genetic effect. Essentially, he modifies the intimate mechanism of heredity by bombardment with ionizing particles; analysis of the modifications yields information of basic interest to genetics. We have here an interesting parallel to some of the methods of particle physicists, who have been notably successful in getting information about particles and particle aggregates by drastic bombardments of various sorts.

Further, there has been a wide application of ionizing particles in medicine, where usually they are produced at considerable depths in a large organism by making use of some primary radiation, such as X- or gamma-rays, which ejects fast electrons from atoms in the irradiated body. Long experience has taught radiobiologists and radiotherapists that certain types of cells and tissues can be inhibited in certain of their functions by smaller amounts of these ionizing particles than are other cells and tissues; and, in cases where the cells or tissues which one wishes to destroy happen to be more radiosensitive than those which must not be destroyed, the medical use of the ionizing particles is termed successful.

In practically all biological applications of ionizing particles to date, including the two examples just cited, it has been technically expedient to irradiate at least a large portion of the organism and all the cells within that gross portion. The primary X- or gamma-rays eject the fast electrons at random as they traverse the irradiated body. And the ejected electrons then travel tortuous paths more or less at random through the adjacent tissue. Much more could be gained in the way of fundamental knowledge of the action of the ionizing particles, and much more effective applications in basic biology could be attained, if we had some device for limiting the paths of the ionizing particles to some relatively small fraction of the cell under study, preferably a known fraction such as the end of a chromosome whose genetic make-up is reasonably well known. To date, studies of this type have been rare, crude, and largely dependent upon structural peculiarities of the cells investigated. The reasons for this lack of exploitation have been technical; it has been difficult to localize the tracks of the ionizing particles. However, the advent of the cyclotron and other sources of fast charged atomic nuclei, such as protons, deuterons, and artificial alpha particles, now make it possible, if one wishes to go to sufficient expense and pains, to limit a parallel beam of particles to a relatively small fraction of a cell of average size. The analytical advantages of such a procedure are obvious.

A SECOND and relatively new application of particle physics is the technique of isotope tracing. This is currently much discussed, and justifiably so, because of its power and its wide applicability in biology.

The principle of the technique has its basis in the ultimate particulate constitution of the atomic nucleus. Disregarding one or two exceptional cases, all the various isotopes of any given element are practically identical in their chemical behavior because their orbital electron configurations are similar. The various isotopes of any element, however, contain different numbers of neutrons in the nuclei. This difference in neutron number makes possible various methods of analysis for the different isotopes. Some of the methods are based simply on the differences in

nuclear mass due to the differences in number of neutrons. The mass spectrograph is perhaps the commonest instrument which works on this general principle. Other methods are based on the fact that some isotopes, because of extreme ratios of nuclear neutrons to nuclear protons, are unstable and therefore exhibit radioactivity, a property which is of course quite distinctive and measurable in absolutely unweighable amounts of material.

Let me briefly illustrate the simplicity, power, and importance of isotope tracing by consideration of a specific example: the physiological function of carbon dioxide in animal metabolism. Prior to the availability of tracer techniques involving the isotopes of carbon, carbon dioxide was universally regarded merely as a final breakdown product of animal biochemistry. This point of view was due to the observation that carbon dioxide was one of the net products of the chemistry of animal cells. In view of this fact and of the further fact that carbon dioxide is universally present in mammalian cells, it was impossible by conventional methods to detect any other role of carbon dioxide, because if a new sample of carbon dioxide were presented to the cells, there was no way of distinguishing a molecule of the new sample from the molecules already present. However, with the advent of artificial radioactivity, a new and radioactive isotope of carbon (C^{11} in distinction to the natural isotopes C^{12} and C^{13}) was made available. A sample of carbon dioxide, some of whose carbon atoms were C^{11}, could then be presented to animal tissues. Since no C^{11} was already present in the tissue, this procedure amounted to "tagging" the carbon atoms in the sample. The result, surprisingly enough, was that some of the radioactive tagged atoms were later found in some of the organic substances isolated from the cells. This showed very straightforwardly that carbon dioxide is not only the end product of some of the reactions of animal chemistry, but that it is actually one of the raw materials. The complexity of the animal metabolism of carbon dioxide was thus demonstrated to be far greater than anyone had realized, and, as a matter of fact, it is only now being unraveled in a substantial way. All this unraveling, needless to say, involves the use of isotopic tracing of carbon in conjunction with the more conventional techniques of biochemistry.

I have chosen the foregoing example not only because of its biological importance, but because of its simplicity. The uses of the isotopic tracers naturally are considerably diversified and have important advantages additional to those brought out in this example. However, I believe this case gives a glimpse of the types of analysis that are made possible by the isotopes and that are impossible without them.

In addition to the radioactive isotopes, certain rare natural isotopes can be used as tracers if samples enriched in these isotopes are made available. The techniques of analysis are, of course, different from those used with radioactive isotopes, but the essential principles and advantages involved in their use are the same. Some of these stable isotopes, such as N^{15} and H^2, have already been used with great success in investigations of the biochemistry of these elements.

For purposes of this discussion, I should like to emphasize that tracer techniques, in both their mental and their instrumental aspects, would be hard to imagine without a foundation of particle physics.

In brief conclusion, I should like to affirm my own attitude toward the particle physics approach to biology as follows. First, it is by no means the only profitable or interesting approach to problems of life. Second, however, it provides concepts of

biological phenomena—and techniques for investigating them—which are of great power and which have demonstrated their value in scientific and practical achievement. Third, it provides fascinating and ever-changing fields in which to spend a lifetime, especially in these days of dynamic expansion of nuclear physics.

One of the best publicized despisers of boredom was Ulysses, into whose mouth Lord Tennyson put the following words:

> All experience is an arch wherethro'
> Gleams that untravell'd world, whose margin fades
> For ever and for ever when I move.

I feel sure that particle physics in these times would provide the ever-changing scene craved even by Ulysses.

FIELD THEORY IN BIOLOGY

By H. S. BURR

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THE study of the development of the central nervous system, using the method of experimental embryology, has done little but confirm the well-known fact that in the brain and spinal cord, with their peripheral connections, is an extraordinarily complex organ system. This very complexity has made the study of the nervous system a fascinating exercise. Much has been learned of the elements which go to make up the intricate arrangements of the parts of the nervous system, but our knowledge is still incomplete. The experimentalists have examined the processes by which development has proceeded from relatively simple primordia to highly organized components. Specific potencies of particular cells have been recognized, and processes by means of which the potencies are realized have been described as organizators, inductors, and the like.

Out of this welter of experimental facts, there has emerged a reaffirmation of the fact, known to all neurologists, that the nervous system is the result of an extraordinarily meticulous and precise design. No matter what part of the nervous system is examined, the preciseness of the design is everywhere evident. Masses of neurone cell bodies are seen to be located in specific regions of the nervous system with great exactness. The nerve fiber processes of these cell bodies connect these nuclear masses over rigidly determined pathways. Variations in these structural relations are virtually unknown, save possibly in some of the minute relationships.

The modern neurologist, standing on the shoulders of many generations of careful investigators, has tended to assume that the design of the nervous system is so well known that, except for minor additions, there is little more to be learned. On the other hand, while the final design seems to be fairly well recognized, there is little information in the literature as to the nature of the forces by means of which this design is realized. A few students have been aware that the design of the nervous system is but a special case of design in the living organism. Many have felt that the solution of the problem of organization in all living things would eventually supply an

answer to the origin of design in the nervous system. The history of biology shows that the few attempts made to solve the problem have been unsuccessful. Scientists have been loath to accept the "final cause" of Aristotle, the entelechy of Driesch, or the *élan vital* of Bergson, for these represent qualitative descriptions of events and not objectively measured correlates of them. They have been dismissed as unscientific. Somewhat better were the embryonic fields of the German experimentalists, notably Spemann, or the physiological gradients of Child. These latter also are unsatisfactory, for the nature of the forces involved is not explicit. Many modern experimentalists tend to rely on tropisms, either mechanical or chemical. It is not surprising, therefore, that many years of study of the nervous system led to the conclusion that the problem must be attacked in a new way. The design of the nervous system is extraordinarily precise. Therefore, the forces which make for that precision must be exceedingly powerful. Moreover, the forces themselves must show interrelationships which control not only local events but also the unfolding of the whole pattern. If this problem is faced squarely the question arises, What kind of forces can be imagined that are capable of bringing about these obvious results? The traditional forces are manifestly inadequate. It would seem, therefore, worth while to turn to a different approach. Design is not unique to living systems but is to be found everywhere in the universe. It is not unreasonable to suppose, therefore, that the same forces which impart design to the material universe might also perform the same function in the living organism.

At this point there appear two different approaches. Many investigators believe that the design of the whole system can be deduced from the complete description of all the entities of which the system is composed. This is the attitude of most biologists who believe that, once the living system is analyzed into its constituent chemicals, the design of the living organism will follow as a consequence. The second point of view, however, maintains that there are relationships between entities not completely derivable from the nature of the entities themselves. It is these relational forces which, in the last analysis, control the directions in which activities move and which, therefore, impart pattern to the arrangement of the entities.

Both of these points of view are implicit in much biological research. The first has, however, dominated the picture to the almost complete exclusion of the second. This is probably due in large part to the fact that the second approach has led largely to the development of unprovable theories. It is at this point that biologists can probably turn to the physicists, for physics tells us that there are in the universe three sets of forces—gravitational, electromagnetic, and nuclear.

Of these, gravitation, although it has its influence on growing things, is not strong enough to establish pattern, and nuclear forces, probably the most powerful known, operate over such exceedingly short distances as to be largely irrelevant to the problem of organization. There remain, then, electromagnetic forces as the determiners of design in the universe. In general, all three sets of forces can be conceived of as definable and measurable aspects of a primary field property of the universe. As such, they lend themselves to experimental analysis and verification.

ELECTROMAGNETIC forces have been known since the investigations of Faraday demonstrated their existence. The mathematical expansion of the field aspects of these experiments, through the analyses of Maxwell, Larmor, and Lorentz, has led to the identification of matter with electricity. The electromagnetic field, in other words,

possesses a vector property which not only establishes the position of all charges within its range, but also gives direction to the movements of the charges. This field, then, has the attributes necessary to establish pattern or design in matter. The importance of the concept has been amply attested by the technological developments of the past hundred years.

Physics, then, has provided a clue to the nature of the forces which impose design on the material universe. But living organisms are also a part of the universe. Unless they are conceived of as special creations outside of the laws governing the rest of the cosmos, subject to the controls of an entelechy, or an *élan vital*, they also must be controlled by electromagnetic fields. Moreover, living beings are made up of the same atoms or charges as are found in nonliving matter. There are no reasons to believe that entities common to both worlds are acted on by different forces. The chemistry of living organisms, for example, differs in no essential way from the chemistry of inert matter, save possibly that the problem is more complex. Moreover, the forces involved in chemical reactions are fundamentally electrical, basically the expression of electromagnetic fields.

It is clear, then, that the heart of the problem of the biologist, the forces which impose design on the living organism in general and the nervous system in particular, is identical with that of the physicist who is faced with the origin of design in the material universe. The solution provided by physics, therefore, should be applicable in biology.

If the above position is accepted, a number of conditions must be met. In the first place, a one-to-one correspondence between the pattern of the electromagnetic fields and the organic form of the living organism should exist. For example, one of the characteristics of living things is symmetry, either radial or bilateral. To be sure, this property is not universal. There are forms of life without it. Nevertheless, it is widely found. The proposal made above would demand a significant, measurable relationship between field and symmetry. There should be a correspondence between an axis of the field and that longitudinal axis which is characteristic of the nervous system of the vertebrate.

In the second place, some method of measuring the electromagnetic fields must be available. In the physical world one property of such fields may be determined by establishing points between which there are steady-state potential differences. Such information does not, of course, identify the sources and sinks of the field, but it does provide an important measure of the relationships between the charges. Knowing the sources and sinks, Maxwell's equations make it possible to define the field and its potential gradients. Knowing the potential gradients, the field can be described without giving specific knowledge of the location of the sources and sinks.

Finally, there must be available a technique by means of which true potential differences can be determined. This condition must be attained through the design of a measuring device that is relatively independent of changing resistance and therefore of changing current flow. Only thus can separation of the dependent variables of Ohm's law be achieved. Only thus can an important measure of electromagnetic fields be made accurately.

But can these conditions be met in the living world? Fortunately, they can, for it has been known for more than 100 years that living systems possess many electrical attributes. That these electrical phenomena are important has been adequately attested by the development of the electroencephalograph, the electrocardiograph, and many electrometric techniques applied to biological problems. Furthermore, there

have been many scattered evidences of the importance of these electrical manifestations in other aspects of the living system. Witness particularly the brilliant pioneer studies of Lund on some of the vital processes of such simple organisms as Obelia and such complex mechanisms as the Douglas fir.

So many signs of electrical activity have been found and utilized that it ought to be possible to relate them to each other under a single assumption. This can be accomplished, first, through adequate theory; second, through logical consequences of that theory; and, third, through techniques for testing the predictions in the laboratory.

Let us assume that electrical phenomena are constant concomitants of the living organism. It should then be possible to determine the existence of potential differences. By definition, such potential differences constitute a field. Can it then be shown that the pattern of these potential differences is correlated with the organic pattern of the living system?

What does this mean? It means, as Northrop and I have suggested, that four questions must be put to nature:

1. Do steady-state potential differences exist in living things?
2. Are they chaotic or do they exhibit a pattern?
3. Does the pattern of the electromagnetic field so measured correlate with the pattern of organic form?
4. And, finally, does the field so determined control the design of the living system in the same way that such fields control design in the material world?

It is clear from the above that the following assumption can be made. Electromagnetic fields determine the pattern of organization in biological systems. The logical consequences of such an assumption demand that these fields be measured with certainty, that they exhibit patterns which correlate with patterns of organic form, and that they maintain a certain constancy throughout the complex flux of chemical reactions accompanying growth and the living process. Local short-time changes in the field might be expected to accompany biological activity of various kinds and therefore yield valuable data about specific biological events. It seems to us, therefore, on the basis of all the evidence obtainable, that the application of the field theory of physics to the basic biological problem might yield interesting and significant results.

The past 15 years have been spent in investigations of the possibilities above-stated. At the outset the development of an improved and simplified technique was necessary. With the help of Dr. Cecil Lane, of the Physics Department of Yale University, and of Dr. L. F. Nims, a physical chemist, in the Department of Physiology, a procedure was evolved which has made it possible for us to answer many questions. Contact with a living system is made through nonpolarizing reversible silver-silver chloride electrodes in salt solution in approximate ionic equilibrium with the solutions of the system being measured. The difference in potential between any two points with which the electrodes were in contact was measured by a vacuum tube bridge, the input impedance of which was 10 megohms. The high input impedance made the measurement of voltage difference independent of resistance change. With this instrument it has been possible to measure potential differences in the microvolt range. The instrument is sufficiently free from drift so that continuous measurements with suitable apparatus can be made over a long period of time. Finally, the procedure has made it possible to study a living system without interfering with its integrity. A very large part of experimental biological research has involved cutting living systems into ever smaller pieces. This destroys at the outset one of the unique properties of living

organisms, their unity. It would take too long to present here all the evidence which has accumulated, but a few of the more important aspects may be interesting.

AT THE outset, it was obvious that there were in general two types of experiments that could be performed. Since the technique made it possible to measure biological systems from a somewhat new standpoint, a great many of the observations were exploratory. The selection of the particular experiment to be performed was largely determined by circumstances. This resulted in studies of many living systems, ranging from bacteria to man. Interesting and important as these seem to be, the more important objective was the relationship between field theory as measured by standing potentials and fundamental biological problems, particularly the problem of biological organization. However, every attempt was made to plan the experiments so that they might give an answer to the four basic questions mentioned above.

The first of these asks if these standing potentials are to be found everywhere in living systems. The answer is, unequivocally, Yes. In every experiment in which measurements were made on living systems in both plants and animals, reproducible standing potentials were found. They were not invariant, but the variation was nearly always within narrow limits. This is not surprising since a living system is a dynamic mechanism in constant activity, and, hence, some degree of variability might be expected. Nevertheless, in general, the measurements were astonishingly constant.

With regard to the second question—Is there pattern in the potential differences?—the evidence was no less clear. Characteristic patterns of potential differences were found in most of the laboratory animals and in man. In the latter instance, the right side of the body was almost uniformly positive to the left side by several millivolts. In something less than 10 percent of the instances this polarity was reversed. Moreover, in any one individual, potentials measured from day to day, over many weeks and months, showed a definite tendency to remain within very restricted limits. Since the technique makes it possible to study the individual without interfering with normal functioning, many artifacts which might be expected to accompany fragmentation of the living system were avoided. Incidentally, the positive polarity of the right side of the body bears no discernible relation to handedness.

Differences between the sexes were also noted. Early in the study it was observed that there were marked increases in the potential difference between the right and left side of the body in women, recurring at approximately monthly intervals. This observation led to a more precise analysis of these potentials, and evidence was collected on both animals and women pointing to the probability that this significant change in standing potential was associated with the phenomenon of ovulation. This was an interesting and unexpected consequence of the theory which still requires additional investigation.

TURNING now to more fundamental aspects of the consequences of the theory, a characteristic of living systems is their capacity for growth. It was necessary, therefore, to determine whether or not these standing potentials change during the growing process. Corn seed, Obelia, salamanders, and mice were explored, and everywhere it was found that during the process of growth and development the standing potentials steadily increased in magnitude to an asymptote. The pattern of these potentials, however, showed only minor variations. In the salamander egg, for example, potential differences between

any two parts appear, but with low magnitudes. During the early stages of cell division and the formation of the blastula and gastrula, there is a steady rise in potential, accelerating with time. This continues with increasing rapidity until the free-swimming larval stage is reached, when potential differences along the long axis of the larva reach a value of 6 or 7 millivolts. The studies on mice reveal that a similar rise in potential occurred in the first third of the animal's life, leveled off during the middle third, and tended to decrease during the last third. During the phases of early differentiation in corn roots and in the developing salamander egg, considerable variability in the stability of the potentials was noted. This suggests that the process of differentiation by itself involves a good deal of biological activity as reflected in potential measurements. It was during these studies of growth that the first real evidence of the existence of field properties was noted.

In the experiments on aquatic animals the technique was so arranged that, once the electrodes were in contact with the animal, the stage on which the animal was placed could be lowered away from the electrodes. The animal was then reversed end for end and raised in contact with the electrodes. This gave a check on the validity of the measurement. Quite promptly it was noted that instead of the potential difference dropping to zero the moment the animals lost contact with the electrodes, a voltage difference could still be read, even though 1.5 to 2 millimeters marked the space between the electrodes and the animal. This means, of course, that there must have been voltage differences in the surrounding solution. Such potential differences are not present when the animal is removed from its aquatic environment. The rate of decline of the potential difference, as the distance increases, is an exponential one. The presence of this phenomenon could be due only to the existence in the living system of a field. At first glance it is somewhat extraordinary that an animal in fresh water or in salt solution can maintain this potential without exhaustion. In spite of the presence of an external shunt, the voltage is not only maintained but increases with time.

At one stage in the development of the salamander embryo, actively beating cilia keep the animal in constant rotation within the gelatinous capsule. If electrodes are forced through the capsule, so that they approach the surface of the contained embryo, regular and rhythmic reversals of potential difference can be recognized as the embryo revolves. Because the potentials at this stage are low, an experiment was performed and photographed in which an older animal was mechanically revolved beneath two electrodes. The potential swings were recorded on an ink-writing galvanometer. Under these conditions the revolving salamander produced an electrical output of alternating character very similar to the output of an ordinary generator, save only in magnitude and frequency. Again this phenomenon could not have been recorded if there had not been in the animal a characteristic field.

The third question was, Is there a correspondence between the pattern of potential differences and organic form? The answer to this problem was sought through a study of the electrical characteristics of embryos of both animals and plants. Again the results indicated a close relationship between the measured potential pattern and the developing organism.

The single-cell egg of the frog and the salamander were explored electrically. Structurally these eggs are radially symmetrical systems, but electrically they are not. Measurements made between the upper pole of the egg and four equally placed points on the equator showed that invariably there was one point which

showed a greater voltage drop than any of the others. This point could be marked and the subsequent development of the egg watched. In every instance, the head end of the embryo developed at the point of greatest potential difference. This electrical asymmetry was present before the egg was fertilized and, so far as the present studies go, showed no change following fertilization. Here, then, is evidence of the existence of an electrical pattern, present before the appearance of any formed structures, carrying on through fertilization and cell division up to the formation of a definite nervous system. The axis of the embryo coincides with the axis of the electrical field. There is, in other words, an electrical pattern, present through the early phases of development in spite of innumerable chemical reactions and definitely related to the pattern of organization of the nervous system. The constancy of this field pattern raises innumerable problems yet to be solved. Since the electrical pattern is present before and after fertilization, there is indicated a positive relationship of the field to the design of the living system and to its genetic constitution.

BECAUSE the problem of form in plants is somewhat simpler than it is in animals, a study was made of the correlation between electrical patterns and form in plant embryos. Cucurbits were chosen because the important studies of Sinnott have provided a structural basis for an examination of any such relationships. Three inbred lines of *Cucurbita pepo* were used. One developed elongate fruits, one round, and one flat. Measurements of the potential differences were made along the axial and equatorial diameters of young ovaries and developing fruits of these three races differing markedly in shape. It was found that the magnitude of the potential differences bears little relationship to the absolute size of the embryo, but there was a high positive correlation between the ratios of the electrical differences and the ratio of the dimensions. Here, then, is another evidence of a significant relationship between electrical pattern and form.

The studies of the electrical properties of plant embryos were sufficiently successful to warrant the extension of the measurements to plant seeds. For, if this field is as fundamental as we have assumed, it would follow logically that there would be significant correlations of electrical patterns with aspects of the living system other than form. Furthermore, since the earlier studies on growth had shown a steadily increasing potential during the growing period, it was thought that there might be a significant relationship between the initial measured potential and the growth capacity of the seed. There have been innumerable studies on corn with a good deal of evidence regarding growth rates and their relationship to genetic background. There was, therefore, a possibility of the discovery of further relationships between potential and genetic constitution. Exploration of these problems was attempted with the cooperation of W. R. Singleton, of the Connecticut Agricultural Experiment Station. The corn seed used were from strains which had been under study for some time. These differed considerably in genetic constitution and more particularly in the degree of hybrid vigor resulting from crosses between them. Four inbred strains were used and three hybrids. The potential measurements were made between the germinal and the micropylar ends of the seed. A statistical analysis of the results showed significant differences in the mean potentials obtained from each strain. One of the strains was a single gene mutant from the parent stock; yet the potential in the parent stock was more than four times that of the single gene mutant. It is surprising that a single gene change should be correlated with such a large change in

potential. The potential differences in the hybrids showed a gradation closely correlated with the measurements of hybrid vigor determined by field studies. One of the hybrids known as "Marcross" is an outstanding early season hybrid. The seed from this strain had the highest potentials of any of the hybrids. Likewise, in the pure strains there was a positive correlation between the results of field studies on yields and the measured standing potentials of the seed. In other words, the potential differences measured were such as to make possible a prediction of the subsequent history of the seed when planted in the field. These studies make it clear that there is a significant relationship between the measurable electrical properties of corn and the genetic constitution of the seed, as well as its hybrid vigor. Here we have the rather extraordinary finding that accurate potential measurements make it possible to predict the subsequent growth history of individual seeds.

Since the above study was dependent in large measure on field data collected before the potential differences were determined, a large number of seeds were measured, placing them in high, medium, and low potential groups. These were then planted under controlled conditions in the field, and their growth and yield studied. Oliver E. Nelson, Jr., Eastern States Fellow at the Connecticut Agricultural Experiment Station, realizing the possibilities inherent in such a study, agreed to carry out the field experiments. During the past three years many measurements were made on corn and the seed allocated to the three groups of potential differences. Each group was then planted in the field under controlled conditions, and at weekly intervals a variety of growth measurements were made. The first summer was, unfortunately, unseasonably dry, with consequent interference with the growth processes in the plants. In spite of the adverse conditions, the high-potential seed developed plants which grew faster, reaching an ultimately greater height, and produced heavier ears than did the low-potential seed. With only one exception, the medium-potential seed produced plants intermediate between the two extremes. The seed from these plants were collected, replanted the second summer, and yielded the same results in more striking form. Seed from these plants were then measured, and the plants from high-potential seed were found to have developed seed with high potential.

In addition to the above results, there was clear-cut evidence that the magnitude of the standing potential was related not only to growth capacity or hybrid vigor, but also to the genetic constitution. More work of this sort needs to be done, but results already achieved provide a promising lead to a quantitative solution of at least two basic plant problems—genetic constitution and heterosis.

One of the logical consequences of the field theory underlying all these experiments demands that variation in the electrical environment should be reflected in small but detectable alterations in the potential differences. It has been shown by Professor Harlan T. Stetson, of the Cosmic Terrestrial Research Laboratory of the Massachusetts Institute of Technology, that changes in the ionosphere significantly affect radio reception.

It seemed worth while, therefore, to design an experiment which would provide continuous records of the changing potential in a living system. If, at the same time, careful records were kept of changing temperature, humidity, barometer, sunspots, and possibly cosmic rays, it might be possible to detect whether or not correlates between any of these variables could be found. A growing tree was chosen for obvious reasons, and quite complete records have been obtained from three varieties

of trees—maple, elm, and oak—in Lyme, Conn., and in New Haven. No correlates were found among temperature, barometer, and humidity of either the magnitude of the standing potential or its polarity. Daily variations of a diurnal sort were found, as were also seasonal changes. Plots of all the data collected suggested the presence of at least three sets of cycles—two 6-month cycles; three 4-month cycles; and a shorter cycle of approximately 28 days, which seemed to be most closely related to the phases of the moon. The data exhibit the remarkable stability of these standing potentials, which, nevertheless, are subject to definite variations. It is difficult, however, to decide whether the determinations are endogenous or exogenous. If they are exogenous, they certainly are not due to local variations in external environment. The possible correlation with phases of the moon suggests a relationship between the local fields of the tree and the general electrical characteristics of the troposphere. The nature of the endogenous forces which might be present, producing variations in the electrical gradients, is quite unknown. It is hoped that by continuing this study over a period of at least ten years, covering approximately one complete cosmic ray cycle, further evidence may be obtained.

Evidence in the literature of experimental biology points to the importance of the electrical properties of living systems. Furthermore, the data suggest that these electrical attributes are of such widespread occurrence that, in all probability, they are of more fundamental significance than has been generally assumed. It seemed worth while, therefore, to study living organisms from the point of view that the electrical manifestations are a sign of a fundamental electrodynamic field. Such a hypothesis demands that in the laboratory there should be one-to-one epistemic correlation between the logical consequences and experimentally verifiable observations. The studies reported above exhibit such a relationship and, therefore, imply that the basic theory has merit. Moreover, since the theory derives from fundamental field theory in physics, it makes it possible to subsume under one heading the nature of the forces which impart design to living, as well as nonliving, systems.

PARTICLE AND FIELD CONCEPTS IN BIOLOGY

By HENRY MARGENAU

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THE preceding articles have demonstrated the fruitfulness of two physical ideas, those of particle and field, in different regions of biological research. An analysis of living structures into component systems and ultimately into particles, which is characteristic of biochemistry, has long been the prevailing method of investigation. As to its efficiency there can be no doubt, and its range has been vastly amplified by the recent discovery, in physics, of new particles and of new techniques for accelerating them. Hence the promise of usefulness of the particle approach to biology is unusually great today.

But the philosopher has noted a strange accompaniment of this trend of investi-

gation: there is a vague though widespread feeling that particle techniques, based on what is often called mechanistic reasoning, cannot solve the ultimate riddle of biological organization. Thus the biochemist has had to deal with peculiarly persistent aberrations such as vitalism and its *élan vital*, with entelechies and biological fields. None of these concepts can be defined as clearly and precisely as the ideas upon which they are grafted and which they are designed to make philosophically more acceptable. In particular the idea of a biological field stood as a challenge to the scientist who tried to understand it with formal precision.

Meanwhile, fields in the physicist's sense were found to surround living organisms and were studied with the use of ingenious electrical recording devices such as the electrocardiograph and the encephalograph. The potentials measured were of the alternating variety and showed interesting correlations with function and with disease. Though their importance was not mistaken, their alternating character precluded identification with the growth-pattern fields postulated to account for biological organization.

The electric fields described by Burr seem to lend themselves more readily to such interpretation. Being constant in time or slowly varying and showing at the same time a most remarkable correlation with growth, the suggestion made by Burr and Northrop that they be looked upon as the scientific counterparts of the vaguer notions mentioned before seems attractive indeed. Furthermore, since all matter is known to contain electrified particles which generate (or are singularities within) fields, this shift of methodological emphasis from rigid particles of the billiard-ball type to electromagnetic fields as determinants of organic behavior will receive the physicist's enthusiastic approval.

Physics, too, has had to struggle with the famous antithesis of particles vs. fields, and its present verdict may be of interest to the philosopher of biophysics. I begin, therefore, with a brief account of the history of the particle-field controversy in physics and its resolution in modern electromagnetic theory.

While the concept of an atomic particle played a role in ancient science, it attained its quantitative importance when Newton formulated his laws of motion and particularly his law of universal gravitation. His theory populated the universe with innumerable mass particles, all of which attracted one another in accordance with precise mathematical equations. Newton himself was noncommittal with regard to the philosophic status of his particles, but his work led later to the conclusion that they were the ultimate, and indeed the sole, constituents of physical reality. This conclusion was reached spectacularly by Helmholtz in his famous lecture before the Prussian Academy of Science in 1847, a lecture which established for the first time the general validity of the principle of conservation of energy on mechanical grounds.

To appreciate the significance of this scientific event, it should be recalled that the five preceding decades had produced convincing evidence for the theory that heat is a form of energy and that both mechanical energy and heat energy are indestructible. Heat had been recognized as kinetic energy of particles. Now Helmholtz was able to prove the conservation principle on the following two assumptions: (*a*) all matter consists of particles; (*b*) all particles interact by central forces, that is, by forces which extend along the lines joining pairs of particles. The success of this proof, coupled with the knowledge that the conservation principle is universally valid, convinced the scientist of Helmholtz' day that the two premises which formed the

basis of the proof must be true as well. Thus the unique importance of particles in physics was established.

Physicists soon became aware that they had committed a logical fallacy in maintaining the premises as well as the conclusions of Helmholtz' proof, for it was soon shown that electrified particles violated assumption (*b*): the magnetic interactions which arise when they are moving cannot be described by central forces. Beside the particle, Faraday and Maxwell found good reasons, therefore, to introduce the *electric field* as a fundamental entity.

Such fields assign to every point of space a unique value of a physical quantity; for example, the electric potential or the vector electric field. It is in the nature of the field that it be a continuous entity extending over a large part of space, in contradistinction to the particle, which occupies a single point.

The structure of the electromagnetic field is determined by the equations of Maxwell, whose theory deals with fields only and leaves, in fact, no room for particles. According to Maxwell's equations, a charged particle is only a singularity in an electromagnetic field, the motion of which is entirely determined by the state of the field at any instant. All matter is described in terms, not of parameters relating to particles, but of quantities like the dielectric constant, magnetic permeability, and so on.

Newton's and Maxwell's theories represented two extreme positions which can no longer be upheld. Maxwell's theory, while eminently successful in a large domain, fails to describe the behavior of light waves in material media. It accounts for reflection, refraction, and absorption of light in a surprisingly elegant manner but renders no account of the phenomenon of dispersion. Lorentz discovered that this difficulty could be remedied only if both particles and fields are introduced into the theory. He proposed what is now called the electron theory of electrodynamics, a theory in which the properties of particles are no longer an immediate consequence of the structure of the field and in which a special hypothesis is necessary to account for the motion of particles within the field. It may be said, therefore, that modern electromagnetic theory has restored both particles and fields to an important place in physics. No longer is reality regarded as either one or the other. If Newton populated the world with hard peas, and Maxwell thought of it as clear soup, Lorentz has converted it into pea soup.

It should be emphasized that in a pure field theory such as Maxwell's, which purports to describe all electromagnetic phenomena and which represents "particles" as singularities in the field, there is complete equivalence between description in terms of particles and description in terms of fields. Strange as this may seem, it is a simple consequence of the fact that a mathematical function is determined by its singularities. The choice of point of view is therefore dictated by convenience, not by physical reality. In more recent theories, however, including that of Lorentz and certain modifications of it, this equivalence is destroyed; the particle has properties of its own which may not be derived from the field.

The field representation of electromagnetic processes, while more abstract than the mechanical concepts involved in the particle explanation, has numerous formal advantages. First of all, it relieves the scientist from the necessity of believing in action at a distance. According to this doctrine (which, to be sure, involves no logical contradiction but has frequently been regarded as objectionable by theoretical physicists), a particle in one place exerts an influence on a particle in a widely different

place, an influence which is not carried through an intervening medium. Newton, in fact, assumed that this influence is propagated instantly from particle to particle. The field concept avoids this difficulty by assuming the influence to be propagated through a field extending between the particles and at a finite velocity. Action at a distance is replaced by contact action.

In particle physics the motion of an electrical charge is determined by the positions and motions of all other charged particles. In principle, therefore, its exact motion can only be found from a knowledge of all the particles in the universe, including the most distant ones. The solution of this problem is practically impossible. With the use of the field concept, however, the situation becomes quite different. While every particle in the universe cooperates in producing a field in the neighborhood of a moving particle, it is this proximate field which determines the motion. To predict the motion, all that is necessary is to measure the field in the vicinity of the particle and then to apply the equations of Lorentz. Motion is no longer dependent on conditions in the far reaches of the universe but on situations in the immediate surroundings of the moving particle.

To illustrate further the usefulness of the field concept, one might refer to a specific physical example. Consider the interaction of solenoids and that of bar magnets. These systems are known to have the same fields, and thus it may easily be concluded that two bar magnets interact in the same manner as do two solenoids, as is found to be the case. To explain the similarity of action by reference to the particles which make up the systems would be difficult indeed. The advantage of the field concept is obvious.

Finally, the simplicity of the field is apparent in the usual statement of Faraday's law of electromagnetic induction. This law is most simply expressed in terms of rate of change of flux, that is, of a certain integral over the field strength. A statement of the law which has reference to particles would appear exceedingly cumbersome.

Introduction of the field idea into biology would seem both natural and fruitful in view of the well-known fact that all processes used in an explanation of biological processes are finally reducible to electromagnetic bases. This is, of course, nothing but a restatement of the triviality that all living organisms contain electrified matter. Nevertheless, it may be instructive to refer to one or two instances in which this reduction can be completely carried out. The laws of surface tension make possible an understanding of a variety of biological phenomena. While physiologists are often inclined to regard the forces of surface tension as something ultimate, it should not be forgotten that these tensions are nothing other than the intermolecular forces called into play between molecules having certain distributions of electrical charge. These forces can be calculated with some precision and may be shown to have their origin in the electromagnetic fields which extend from the charges in one molecule to those in another.

Osmosis provides a similar example. The usual explanation involves ideas of small and large molecules and rarely considers the cause of molecular size. In fact, the sizes of molecules are conditioned by the field interactions between electrified particles of which the molecules are composed.

Other examples could easily be cited. To explain nerve conduction, electrical motions are invoked, but the theory has not yet attained a form in which complete reduction of the processes to definite electromagnetic phenomena is possible. All these instances argue for probable usefulness of electro-

magnetic fields as explanatory concepts in biology.

After this general discussion of the status of fields and particles in physics we turn our attention to the specific findings of Burr. To attempt to derive the steady fields which he observes by solving Maxwell's equations in detail would be a hopeless undertaking in view of the technical complexity of the problem. Furthermore, since the precise positions of the charges within an organism are not known, the boundary conditions for the solution of this problem could not be ascertained. The best one can do is to assess, in the light of experiences gained in solving Maxwell's equations for simpler conditions, the reasonableness of the observed fields; and here it may be said that they do not contradict the theory in any evident respect. It is true that the potentials measured must satisfy Laplace's equation in the saline medium surrounding the organism, and a cursory examination of the measurements indicates that they do. Also, the presence of currents in the surrounding medium must alter the metabolic rate of the animal, but rough computation shows that the change is negligible in comparison with the normal metabolic rate.

Perhaps the simplest picture in terms of which the results may be presented is that in which the organism is replaced by a very large number of batteries connected in such a way as to give rise to the measured potentials. The elements are cells which are known to possess varying ionic concentrations and thus give rise to electromotive forces. How the cells are connected is at present a matter for speculation with respect to which the data have nothing to say.

The existence of the fields and their compatibility with the laws of physics is not surprising, but the discovery that they are correlated with axes of growth seems of utmost importance both in its practical implications and in its meaning for the development of theory. It is at this point that Burr's work departs from the lines of traditional physics. For the only explanation which classical physics seems to offer is in terms of electrolytic action, a rather lame proposal in view of the observations. Electrolysis might, to be sure, account for the deposition of matter in the region where the head of the salamander is formed (this being the cathode with respect to the external circuit) but one hesitates in venturing so crude a suggestion. The correlation of electromagnetic fields with growth must therefore be regarded as a novel discovery which present-day physics does not completely elucidate.

The field approach to biophysics raises several very interesting logical and philosophical questions to which brief reference will here be made.

The first concerns the logical relation between the batteries of our simple picture and the field. Certainly the two are closely related in the sense that if the state of the batteries were known the resulting d.c. field could in principle be calculated. The reverse, however, is not true. Knowledge of the fields does not suffice for a prediction of the detailed ionic states of the batteries nor of the connections between them. This lack of reciprocity of the logical relation between particle configurations (batteries) and the field may be of considerable scientific importance. If the problem of growth were connected with the state of the batteries, its solution would seem exceedingly difficult. According to Burr, it is tied to the state of the field, a knowledge of which can apparently be gained by direct measurement and without recourse to complicated particle configurations. From a methodological point of view this seems fortunate indeed and may hold promise of simplifying biology.

There is also the question as to the causal relation between the batteries and the field, a question not explicitly dealt with in the preceding article; but Burr and Northrop, in earlier publications on what they call their electrodynamic theory of life,[1] appear to have taken the attitude that the field may be primary in a causal sense. In accordance with this view one would have to think of the field as not entirely conditioned by the particulate state of an organism, but indeed as an agency connecting the batteries of our simple picture in a manner conformable to the external field. The origin of the primary field is somehow left unexplained, and the teleological questions which gave rise to the earlier field theories of biology have been side-stepped. It would be interesting to have Professors Northrop and Burr discuss this matter in greater detail. The physicist, at any rate, has very little to say about it.

It is a well-known fact that the static field surrounding a closed surface such as a salamander egg is entirely determined by the values of the electrostatic potential at all points on the surface. This leads to the curious consequence that the fields measured by Burr, fields which determine the growth of a three-dimensional, complicated, and finely organized object such as the salamander head, should themselves be completely determined by potentials on the *surface* of the salamander. A three-dimensional structure appears to be mapped on a two-dimensional surface. While this is by no means impossible, it is a result perhaps more surprising than the congestion of a large number of biological traits into the small space of a chromosome.

On the other hand, the proposals under discussion have also been subjected to criticism of the following sort. If the electromagnetic field is a growth-controlling factor, its precise character, its minute variation *within* the organism, should be important. External fields are at best merely the tail end, or the asymptotic part, of the significant fields within. Why, then, focus attention upon these secondary agencies? This is not a serious objection. For there are phenomena in atomic physics where a very similar situation exists without occasioning doubt. For example, in the scattering of particles by atomic fields, it is the *asymptotic* part of the wave function which determines the results. If there were a way of finding this asymptotic part without knowledge of the function near the atom, it would indeed be acceptable and a welcome simplification of the problem.

Before concluding, I may perhaps be permitted to inject an idea which, though it is only loosely related to the subject of the preceding articles, may be of interest in connection with the larger problem of biological organization.

Most of the points raised thus far deal with the question of mechanism in biology. The fields discussed are meant to implement the procedures of mechanistic physics to make them account for organization and growth. Modern physics, however, contains elements which are definitely non-mechanistic, elements which lend themselves, within limits, to an understanding of some processes similar to those of biological function. Foremost among them is the exclusion principle of Pauli; it may be shown to have an interesting bearing on what has been said. The principle is an abstract one requiring a certain symmetry of the states of a complex system. It is non-mechanistic in the sense that it does not allow the properties of the entire complex to be derived from the properties of its parts. In its simplest form it shows that no two elementary particles can be in identical states.

Its application to atomic systems is well

known. For example, the orbits of the electron in the hydrogen atom are determined by dynamical laws. When a second electron is added to the nucleus to form a helium atom its motion is no longer fixed by dynamical laws alone but is dependent upon the fact that another electron is present. Crudely, one might say that one electron "knows" of the presence of the other and adjusts its motion accordingly. This anthropomorphic statement is, of course, not to be taken literally; it is meant merely to imply that the effect of the exclusion principle is the same as would be the effect of such "knowledge." In the structure of heavier atoms this adjustment of the motion of one electron to the presence of others not implied by the dynamic laws of motion but imposed by the exclusion principle is of utmost importance.

Emergence of new properties is a common phenomenon in modern physics and one which can also be traced to the same principle. For example, when two hydrogen atoms interact they will either repel or attract each other. Once two atoms have formed a molecule and this molecule is approached by a third atom, the alternative of attraction or repulsion no longer exists. The two atoms will always repel a third. Thus a new property, invariable repulsion, or saturation of attractive forces, has superseded the former alternative. Finally, there is a host of other effects, frequently termed cooperative effects by physicists, to which the exclusion principle gives rise but whose inclusion here would lead too far afield. The general importance of this principle has been discussed elsewhere.[2]

While this purely formal element of modern physics leads us a considerable distance toward an understanding of some of the crucial biophysical problems, it does not of course promise to solve them all. It does, however, provide a beautiful example showing the fruitfulness of a single change in physical methodology, and it may not seem unreasonable to suppose that further principles of symmetry of this general type may some day shed light on a larger field of problems.

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THE COPPERHEAD

By Paul D. Harwood

I met you where the sun
Had warmed a patch of forest floor;
Tricornered spots of brown that often fused
Along your back of lighter tan
To form crossbands of hourglass shape
So close a match had made to patterns cast
By sun and shade in last year's leaves,
That I had overlooked you till you moved.

And many times, I found you under logs,
Retreating not from foes,
But from the cold a norther brought in March,
Or from the harsh, dry heat of mid-July.
My moving mass disturbed you not at all,
You merely watched with lidless eyes,
And tested with your black-tipped tongue
The littered space between.

And when I stepped too close,
Your head drew back
In preparation for defense,
The while your sulfured tail a warning rattled
On a dry and withered leaf.
An inch or so above the forest floor,
Your still head swung,
Alert with flashing tongue.

I left you crushed and broken,
The marvel of your body distorted by a blow,
For I have never found the strength
To leave you undisturbed.
The blow has marred my memory;
Yet in it lies a forceful image
Of simplest patterns and somber dyes
That bore a scaly beauty to my scanning eyes.

OKINAWA AND ITS PEOPLE—I*

By PAUL E. STEINER

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OKINAWA SHIMA, the largest island in the Ryukyu (Luchu, Loochoo, Nansei, etc.) Islands, was invaded by United States naval and military forces on April 1, 1945. The actual invasion was preceded by a prolonged but intermittent bombardment by planes and ships which began in October 1944. Our possession of this island was necessary to deprive the enemy of an important step in his chain of islands and to serve as a base for further advances. Because of the nature of the defense, particularly in the heavily populated southern end of the island, the combat was on a large scale. As a result, the country was heavily damaged and the population extensively dislocated. Advance information on these islands was, in many respects, defective because they had been isolated from the main commercial, cultural, and tourist currents. Because of their isolation, however, they had developed in certain cultural respects along independent and original lines. During reconstruction the American influence has been so great that life as it existed before the invasion is not being reconstituted. Even before combat stopped marked changes were observable in the people and their life. Some observations on the old Okinawa might be of interest to workers in the biological and social sciences.

Okinawa is a tropical island about 60 miles long and 3 to 8 miles wide, lying between 26° 04′ and 26° 53′ north latitude and 127° 38′ and 128° 20′ east longitude. It is roughly equidistant (350 miles) from Formosa, the China coast, and Kyushu, the southernmost large Japanese home island, and slightly farther from Luzon. It lies in a northeast to southwest axis. A large peninsula, Motobu, projects about 10 miles from the northwest side; a smaller one, Katchin or Katsuren, from the southeastern shore; and another, the Chinen, from the southern end. The island shows volcanic and coralline origins, the latter predominating. The northern two-thirds is rugged, rocky, and forested; the southern third is rolling and broken, but wherever possible it is used for farming and it contains most of the inhabitants. Shallow valleys, divided into small plots for farming, ravines, and coral outcroppings clad with flat-topped pines and cycads characterize most of the island. The highest elevations are about 1,600 feet in the northern sector and 500 feet in the southern. The coast line is protected by reefs in nearly all places. Several beaches with coastal flats up to about a mile in depth are found, but generally there is an abrupt rise to the interior plateau behind a narrow coastal region. Innumerable small springs and a few large ones are scattered over the island. Despite a heavy rainfall, no large streams are found because of the narrowness of the island and the extensive use of water-impounding and -utilization devices, which hold the water near its point of fall.

The climate is good. Temperatures are moderate: the maximum in summer is over 90°, and the minimum in winter is in the forties; the means range from 60° in January to 83° in July. There are no

* This article has been released for publication by the Bureau of Medicine and Surgery and the Security Review Section of the Office of Public Information of the U. S. Navy. The opinions and views set forth are those of the writer and are not to be considered as reflecting the policies of the Navy Department.

sudden variations in temperature from day to day or from night to day. The humidity is high at all seasons. The annual rainfall of about 80 inches is distributed throughout the year from about 10 inches in May and June, the wettest months, to 4 inches in December. Thunderstorms are uncommon and mild. Typhoons, up to two or three per month, may be expected at any time from June to December, but only one to three of them each year are severe. They usually come from the southeast, arising in the region of the Mariana, Marshall, or Caroline Islands; gusts of wind may reach velocities of up to 200 miles per hour during such storms. Living has been admirably adapted to this violent natural phenomenon, as well as to earthquakes, which can be felt about once a month.

The population of Okinawa was given as 460,000. The racial origin of these people is obscure; their earliest recorded history is said to date from 605 A.D., although their folklore goes back several thousand years. When asked whether they are Chinese or Japanese, they invariably reply that they are neither but Okinawans. It is believed that engrafted on the original racial stock, the Ainu, white (Caucasoid) people common to these and the Japanese Islands, were liberal admixtures of Chinese, Korean, Japanese, Filipino, and Malayan peoples. To these were added in a few communities in more recent centuries, very small amounts of Portuguese, Spanish, and possibly other European strains.

The Okinawans were mainly rural people, and the chief occupation was agriculture, followed by fishing. Several dozen villages and towns and a few cities had grown up. The prewar populations of the larger places were said to be as follows: Naha, 65,000; Toguchi, 20,000; Shuri, 17,000; Nago, 13,000; Haenna, 8,000; Itoman, 6,700; and Yonabaru, 5,000. Until recent times communications throughout the Ryukyus were poor. About five different dialects were found in the Ryukyuian language, three of them on Okinawa. Most of the older people could not speak Japanese, which was introduced in the schools some years ago as part of the program of Japanization begun about 1875 and intensified after 1900.

During the campaign I had the opportunity to perform a series of 150 necropsies on native Okinawans. From these observations were derived anatomical and pathological data which I have described in "Anatomical and Pathological Observations in Necropsies on Okinawans." (In Press: *Archives of Pathology.*) Further information on life in the islands was obtained by submitting, in interviews, about 400 questions to eight Okinawan physicians from various towns and to a number of English-speaking residents. Most of the questions concerned medical problems, but some of them dealt with social, economic, religious, and cultural conditions. These two sources of information were supplemented by personal observations during travel, with interpreters, about the island. Historical and religious information was obtained from several other compilations and from native Okinawans.

History. The earliest recorded event in Okinawan history dates from 605 A.D., when a trading mission sent by the Emperor of China tried to obtain information about the Ryukyus and make a trade agreement, both without success. Prior to that time historical events are legendary, and the origins of the people and their land are closely connected with their religious traditions inasmuch as the religion is a system of ancestor worship, which tended to preserve such information.

According to legend, the people and the land originated long ago from chaos when the sun-god, Teda-Ko, dropped from heaven Man and Woman, who gave origin to the first male, Shireku (Shineko, Shinireku),

and the first female, Amamiko (Amanikye), who created the land and the people. According to an alternate myth, the first people arrived in a storm from the islands of the South Seas. At what date the first people came is not known, but some villages claim that they have occupied their present sites for 3,000 years. The residence of the first people is variously described as at Kadaka-shima, Kori-jima, or Nago. Kadaka-shima seems to have had official support for its claims because the former kings of Okinawa visited the island for worship and prayer each February. It is a small island located in the entrance to Nakagusuku Wan off the east coast. (This has been renamed Buckner Bay in memory of Lieutenant General Simon Bolivar Buckner, Jr., Commanding General of the U. S. Tenth Army, who lost his life from artillery fire south of Naha on June 18, 1945, in the last days of the campaign.)

Modern ideas regarding origins are less romantic. Geologically, the island shows volcanic and coralline origins; racially, the people are considered to be a mixture of Ainu (Caucasoid), Mongoloid, and Malayan strains; culturally, they exhibit Chinese and Japanese influences engrafted on an older, possibly indigenous, culture.

According to legend, the Okinawans in turn gave rise to the Moro peoples inhabiting Mindanao in the Philippine Islands when a sailing vessel was wrecked in a storm. Being unable to build a new ship, they were forced to remain. In support of this story they point to similarities in the languages and in the religious custom of praying to ancestors represented on the name plates in the family shrine.

In 1945 four ancient castles were found, located at Shuri, Nakagusuku, Yontan, and on Katchin Hanto near Haebaru and built on strategically located, rugged hilltops which commanded wide views. The construction was of hewn stone fitted into thick, massive walls filled with rubble. They were said to be about 500 years old but, even so, to be the successors to an older series of prehistoric castles which have disappeared. They are survivors of the feudal days. Shuri Castle was the last dominant castle, the ancestral home of the last Okinawan kings, and, more recently, an administrative and governmental center.

Since the dawn of recorded history the Okinawans, although nominally independent, were influenced by both China and Japan and at times paid allegiance and tribute to both. At one time Okinawan sailing vessels carried on widespread commerce with the Asiatic mainland and the islands of the Western and Southwestern Pacific. The island was first brought under a single rule in 1187 by one Shuntem-O, a king with Japanese ancestry. Thereafter a succession of kings ruled for 700 years. During part of this time China, particularly under the Ming Dynasty (1368–1644), exerted the dominant influence. Buddhism was introduced from China in the fourteenth century, and the Okinawese princes were educated in China or by Chinese tutors.

When Japan adopted the closed-door policy in 1638, Okinawan commerce, which was important, was stifled and an agricultural economy was forced on the island. Commodore Perry visited Naha on his way to and from Japan in 1853–54 and he urged on the American Congress that coaling and observation posts be established there. In 1875 Japanese troops occupied the island, and tribute to China ceased. The king was reduced to viceregal rank, sent to Tokyo, and Okinawa was incorporated into the Japanese Empire. Since then efforts to Japanize the island have been intensified.

Anatomy. The Okinawans are small, dark, well-proportioned, Oriental people. The skin is usually dark brown. This deep pigmentation is no doubt due partly

to the tropical sun. A few yellowish persons are seen and rarely one who is very light-skinned. I saw a fair-skinned child on northern Okinawa and another on an adjacent island, Haenza Shima. Both children otherwise resembled the usual natives. I was told that such people were not rare; they are attributed by the natives to European rather than to Ainu mixture. The hair is straight and black, and both sexes have on the average much less body hair than Americans. The body length of adults in a small series was 60.5 inches for men and 55.8 inches for women. Children are correspondingly small at the different ages. Young women are sometimes plump, but obesity is not found at any age in either sex; these people therefore appear quite muscular. The body is slender to stocky; the legs are moderately short, and an appearance of bowlegs sometimes seen is usually because of wide placement of the femoral heads rather than bowing. The carriage is erect, and the bearing, especially in women, is graceful. Heavy labor in the fields is regularly performed by the women, and they habitually carry burdens on their heads. The feet are broad across the toes; the great toe stands apart and is trained for grasping.

The facial features are Mongoloid, with slight to moderate development of the medial epicanthic folds and a moderately broad face. The appearance varies between two extremes: Some have a flat or a concave broad face, a large mouth, and a broad nose with sunken bridge, associated with a stocky body; others have a narrower face, with a straight, thin nose, and a slenderer body.

The internal anatomy shows several points of difference from Americans and Europeans. The organs are small in proportion to body size, with two striking exceptions: the spleen is smaller and the pancreas larger than would be expected. In Americans the normal spleen is slightly larger than the pancreas; in Okinawans the normal pancreas is about one-third larger than the spleen, and it is nearly as large as that of Americans. The colon is relatively greatly elongated, so that it has several extra coils. In the hepatic flexure it frequently exhibits an S-shaped curve, the transverse colon lies very low, and the sigmoid region forms a long, pedunculated loop. The suggestion is made that this elongated colon represents an adaptation to the prolonged use of a predominantly vegetarian diet, which has a bulky residue, analogous to that of herbivores. On dissection, the excellent muscular development becomes conspicuous because of the small amount of adipose tissue, both subcutaneous and deep.

Taken as individuals, Okinawans are indistinguishable from the Japanese; considered as a group, however, the Okinawans are darker, shorter, stockier, and hairier.

Pathology. Okinawa appears to have been, on the whole, a fairly healthful place. In comparison with the United States the infectious diseases were more important and the degenerative diseases were considerably less common, so much so in fact that the reasons therefor merit further investigation in the hope that etiological factors may be disclosed.

A few infectious diseases were less common than in the United States. These included the streptococcic diseases (notably scarlet fever) and their relative, rheumatic fever. Mumps was said to be rare, and most of the population seemed to escape whooping cough.

On the other hand, intestinal helminth infections were very common, though they seemed to cause little trouble. In the bodies examined 44.7 percent showed *Ascaris lumbricoides*, 34.7 percent had hookworms (chiefly *Necator americanus*), and a few of them were infected with *Trichuris trichiura*, *Enterobius vermicularis*, *Strongyloides stercoralis*, and tapeworms. Tubercu-

losis, the dysenteries, the diarrheas, and pneumonia were common and constituted the chief serious medical problems. Filarial infection, as revealed by the presence of microfilaria in the blood, had a high incidence, but its sequel, elephantiasis, curiously, was uncommon, indicating a benign infection despite the fact that the organism is said to be *Wuchereria bancrofti*. About a thousand lepers, collected from the Ryukyus, were under treatment in a leprosarium located on Yagachi Shima, a small island off Okinawa. This disease was stated to be decreasing. Malaria was not a big problem; a few cases formerly occurred annually, chiefly in the northern part of the island, where, however, after the invasion with its disruption and overcrowding an epidemic ensued among the natives. An outbreak of encephalitis, believed to be Japanese B encephalitis, also occurred; this was said by the native physicians to be an almost annual event. It was the only disease found which is not present in the United States, although its near relatives are entrenched here. The island appears to be heavily contaminated by a virulent form of tetanus bacteria, as indicated by the history of numerous cases in the past following childbirth and trauma and by the large number of cases in natives which followed war injuries. Dengue or a dengue-like disease was said to cause epidemics at intervals of a few years. Measles and conjunctivitis appeared in epidemics almost annually.

Yaws, schistosomiasis, leishmaniasis, tsutsugamushi, typhus, cholera, plague, anthrax, smallpox, rabies, and some other diseases did not occur, according to the local physicians.

The degenerative and retrogressive diseases appeared to be relatively rare. Arteriosclerosis, lithiasis (cholelithiasis and renal lithiasis, but not bladder stone), hypertension, malignant tumors, heart disease of all types, disorders of the endocrine system such as *diabetes mellitus* and hyperthyroidism, and nodular enlargements of the prostate, thyroid, and mammary glands appeared to be relatively less common than in the United States. The causes therefor were not obvious, but dietary and hereditary factors and a placid, low-tension but physically vigorous life, as well as the equable climate, may be important.

The sick were treated by home medication and by doctors who were in private practice. Most of them were native Okinawans, sons of well-to-do familes who had been educated in Japanese medical schools. They were, on the whole, well trained to recognize and treat the diseases encountered. Most of them did only minor surgery except that they treated fractures and drained infections; major surgery was generally referred to a few doctors who specialized in this work. Several examples of group practice had sprung up about small hospitals, which were found in the larger cities. Licensed native practitioners, who had been trained by serving time in a hospital, were also in practice. Childbirth was attended usually by relatives, less commonly by midwives, and by physicians only after trouble was encountered. Infant mortality was fairly high. The oldest inhabitants were said to be about a hundred years of age, and many lived past seventy.

The people possessed a great lore of herbs and medicines; most of their remedies were cathartics or so-called blood purifiers and strengtheners. Pharmacies, located in the larger towns, had a fair assortment of some of the modern drugs.

The Home. The homes, each a compact unit enclosed by a wall or hedge, were located either on the farms or clustered together to form villages and towns or, more rarely, cities. Whether single or in groups, they usually occupied land which was not of the best quality for farming

because of outcropping rocks or steepness. The homesites were frequently cut back into a slope to give protection against typhoons by embankments. The homes were connected with the farms by footpaths or by narrow dirt or cobblestone roads just wide enough to permit passage of small two-wheeled carts.

The buildings comprising the home were small, and the plan of their arrangement was fairly uniform. They were enclosed in a level square or rectangular space by a dense hedge of giant grasses, shrubs, and trees which grew directly from the level ground, or from a dirt wall, or from the embankment at the edges of the area if it was cut back into a slope. Wealthy people had a stone wall of fitted rubble or hand-hewn coral blocks. The hedge or wall, regardless of its composition, served to keep out prying eyes and to lessen the force of typhoons. The home compound had one entrance located in the center front. This was not guarded by a gate but by a short segment of wall or hedge several feet longer than the width of the entrance and set a few feet interior to it. Thus it was impossible to look directly into the home or to enter the compound on a straight line. It was necessary to turn to the right or left around the end of the short inner guarding wall, whose function was the same as that of the main wall. Legend gives its original purpose as that of keeping evil spirits out of the home since, supposedly, they were unable to negotiate the turns. The house nearly always faced south to take advantage, being open in front, of the cooling breeze which prevailed from that direction in the hot summer months and to admit the low-angle rays of the sun while keeping out the prevailing northern breeze during the winter months.

The following arrangement could be seen from the entrance of the typical rural compound: a small stable to the left, storage barn to the right, and a larger, rectangular building of better quality extending nearly across the back of the compound. This was the house. At its left end stood a small, partly sunken, thatched pit constructed of hand-surfaced, fitted coral blocks. This structure was a combination pigsty and latrine, whose important function was the conservation of human and animal excreta for use as fertilizer. A well from which water was drawn with rope and bucket was located in any part of the compound; nearby was a small outdoor pool. Part of the compound was paved with coral blocks in the homes of the affluent. A few small fruit trees or flowering shrubs, usually hibiscus, along the wall completed the typical arrangement. The position of the house was constant, but that of the storage building, stable, or pigsty-toilet was sometimes reversed. An upright, cylindrical stone or concrete cistern, about 30 inches in diameter and 6 feet tall sometimes stood between house and stable, collecting rain water from the roof of each. In the towns the storage barn in the right front corner was not uncommonly replaced by a building used for ceremonial occasions or for housing guests.

The house was usually built of wood with a mortised, flexible frame and thin board paneling inside and out. The roof was usually grass thatch, although red tile roofs were not uncommon. A brick or concrete lion or tiger head on the roof guarded the home against evil spirits. The stable, pigsty, and storage barn were usually thatched on bamboo attached to a rough timber frame. Most thatched buildings had heavy upright corner pillars of hewn coral securely fixed in the ground, between which were walls composed of rubble.

At worst, all the buildings were made entirely of thatch or with mud and rubble sides and thatch roof. At best, they were all built of hewn coral or wood with red

tile or even molded concrete tile roofs. Rarely, the house, or stable, or both had two stories.

The open front and right side of the house could be closed by sliding wooden panels, which ordinarily were out of sight. A second set of panels, constructed of latticed wood covered by rice paper, was sometimes placed about 2 feet interior to the outer set. Additional protection from sun and rain was given by wide, overhanging eaves, which were supported by a row of round poles, cut from the trunk of small eucalyptus trees in such a way that the spreading roots at the butt provided a wide, secure base. Eucalyptus wood, being resistant to termites, was used whenever possible in the framework of the house.

The kitchen was in the left end of the house. It had a dirt floor and was equipped with mud, tile, or stone hearths, cupboards, pottery jars for storage, and various utensils, mostly of home manufacture. There was usually no chimney, so the walls and roof were blackened with soot. The smoke was conducted under the roof through the garret over all parts of the house. This smoking, which gave a characteristic odor to the houses, was thought to preserve the wood from termites. Dried straw, twigs, and sugar-cane stalks were used for fuel.

Small homes had one living room; larger ones had two or three in a row across the front, in addition to several small rooms across the rear used for storage of food, weaving materials, and unused furniture. The main living room was called the shrine room because recessed into its rear wall was the family shrine. If the house was fortunate enough to have a second room, it was placed to the right of the shrine room. This was known as the porch room because it was open on two sides, front and right. If the house was favored with a third room, it was interposed between the shrine room and the kitchen and was used mainly for eating. All the main rooms except the kitchen could be converted into a single, large room by sliding back the disappearing panels. This was done during the day and for ceremonials. At night these panels were used to subdivide the space into sleeping rooms, and the family slept on thick straw mats laid on the wooden floor. The sleeping gear was stored in a built-in closet during the day.

The furniture consisted of the shrine, a low table, several small straight chairs, and small trays. Most of the furniture was lacquered, a finish suitable to the damp climate. On the wall were usually found framed pictures of any or all of the following: Emperor Hirohito, members of the family, Thomas Edison, Napoleon, and Abraham Lincoln. The large houses had a special seat reserved exclusively for the rare visits of royalty.

There were no modern plumbing and sewage systems. Houses were well ventilated, being partly open, but uncomfortably cool during three winter months because of lack of heating facilities. Artificial lighting was not needed because of the habits of the people.

The Family. A family was begun by a marriage ceremony, which was usually secular and participated in by both families concerned. It lasted from one to three days. The groom was usually about twenty-five years old and the bride about twenty; the extremes were eighteen to thirty-two and sixteen to twenty-five years, respectively. The average age at marriage, formerly younger for both sexes, has increased, owing to more years spent in school and in compulsory military training. The marriage was formerly arranged by the two families, sometimes through a middleman, but in recent years there has been an increasing tendency for young people to choose their own marriage partners—so-called love marriages. Mar-

riage enforced because of pregnancy was said to be rare. The custom of betrothal by presentation of the *tisaji*, woven from cotton in the form of a towel by the girl for the boy, was abandoned about 30 years ago when cheap machine-made Japanese cotton cloth made local cotton raising and weaving obsolete. Acceptance of this gift indicated that the male realized that alone he was incomplete, a defect which the girl by making the gift indicated she could remedy.

The newly married couple lived with the groom's parents if he was a first son; otherwise some other home was arranged. The complete typical family consisted of the paternal grandparents, the oldest son and his family, and any unmarried sisters from all generations.

Okinawan culture was patriarchal and patrilinear. The oldest male in the family, usually the grandfather, ruled with the power of a dictator. At his death the rule descended to the oldest son, provided he was twenty years old; otherwise it fell to the grandmother until her son reached his majority. Property was inherited in the same way as power. The oldest son inherited nearly everything, but he could share with his brothers if he wished or if the father required. This failure to divide the property explains how such large homesteads have been built up in a country where earning power and resources are so low; they represent the accumulated wealth and productive energy of many generations.

Formerly the backs of the hands of brides were extensively tattooed with blue-black pigments. This was a form of branding to announce to all that she had a husband. According to rumor, it was directed mainly against foreigners who might wish to molest the wife. This custom was abandoned about 30 years ago and in 1945 was seen only in women over fifty years of age.

The married couple soon had a series of children. The average number was said to be five or six; the extremes were none to twenty, and families with ten children were common. The babies were generally born two years apart, the mother nursing a child until the next one came along, which was sometimes as long as three years. Sterility was blamed on the woman, and it was the main cause for divorce on the island. Childless couples sometimes adopted a child. A man rarely had a second wife or concubine, and any such were kept secret and apart from the family home.

The women did all the work in the compound and much of that in the fields. They cooked, cleaned, wove cloth, and made clothes; collected, dried, and stored food; milled the grain; fed the animals; and planted and harvested the sweet potatoes and other produce, carrying burdens on their heads in baskets which they had woven. The men supervised and helped in the fields, did the skilled work in connection with building, construction, and irrigation, and fished. Many farmers were engaged in part-time work away from home.

The men controlled the finances and kept the money, but there was one exception to this custom. At Itoman, a fishing village where a few Portuguese are believed to have settled centuries ago, it was said the women kept a separate purse. Here it was even possible for an unmarried daughter to earn and retain money to be used as a dowry, which sometimes took the form of a new fishing boat for the bridegroom.

Diet. The main articles of diet were sweet potatoes, rice, and various types of beans, including soybeans. These were supplemented by green vegetables of many kinds, millet and barley, meat, fish, and fruit. Squash, tomatoes, radishes, carrots, eggplant, cabbage, chard, onions, asparagus,

and other vegetables were found in some gardens but by no means on every farm. They were eaten either raw or cooked, as were many types of green leafy plants. Taro, tapioca, and sago were produced for home consumption. Sago was made from the ubiquitous cycad, *Cycas revoluta*, which was used for food in several forms after treatment to reduce its inherent primary toxic factor. Fruits were not used in large amounts; the commonest varieties were a small sour orange, a small banana, papayas, and a few peaches. Black or brown sugar made from the cane grown on nearly every farm was eaten as such or made into confections; it was not used to sweeten food as is the Japanese custom. The natives considered it profitable to sell sugar and buy rice.

The chief source of proteins was soybeans. Most families slaughtered one or two hogs annually, usually at the New Year. The pork not eaten fresh was preserved with salt, refrigeration not being available; goats were also raised and used for meat by most families; chickens and ducks added a little variety. Fish was eaten several times a month in most homes. Beef and horse meat were rarely available to the average family. Milk from goats or cows was used only by those who were ill and by babies whose mothers had insufficient milk. Cheese was repellent to the Okinawans.

Dietary fats were chiefly lard rendered at home and soybean oil imported from Manchuria. Attempts, not very successful, were made to make beef tallow more usable by mixing it with soybean and other oils. Small amounts of oil were obtained from various types of nuts and seeds, including cycads, again without great success. Olives were not grown on the island, and coconuts, which are so important elsewhere in the islands of the Pacific, were very rare.

The diet appears to have been overbalanced by carbohydrates and to have had an excessively bulky residue. Superficially, it seems that it must have been deficient in proteins. However, when it is judged by fertility, longevity, incidence of disorders of metabolism, and evidence of deficiency diseases, it stands up very well. Prior to the war it protected against most infectious diseases except dysentery and pneumonia. It certainly was not conducive to a rapid rate of growth or ultimate large stature, criteria commonly used in judging diets in America until recently. Some cases of war edema were seen but only after a long period of inadequate food intake consequent to the disruption of normal living conditions by the war. The natives preferred their food to ours, but this is grossly unfair to us because they were judging mainly by our K rations.

[To be concluded]

THE INTERRELATIONSHIPS OF ENZYMES, VITAMINS, AND HORMONES

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VARIOUS activities of the living organism need for their continuance the mediation of enzymes. Such problems as those connected with digestion, tissue synthesis and repair (including growth), reproduction, nerve conduction, muscle contraction, and metabolism (changes which foods undergo in the organism) are all under the control of enzymes. What, then, is an enzyme?

The answer can be illustrated by a specific example. In the stomach we find a substance, pepsin, which acts chemically on proteins in our foods, transforming them from complex chemical molecules (proteins) into relatively simple ones (largely amino acids). The actual amount of this pepsin is minute; yet it has the ability to transform enormous quantities of protein into amino acids without, apparently, being used up in the process. Pepsin is our conception of an enzyme; and this conception is in accord with the view that the enzyme is a catalyst—a substance which initiates or, still better, accelerates, a chemical reaction without itself undergoing any apparent change. Since the enzyme does not undergo any apparent change, a little of it goes a long, long way.

Pepsin is merely one example of hundreds of enzymes in the body, each with sharply defined functions—some, like pepsin, acting on proteins; some, like ptyalin in the mouth, acting on carbohydrates; some, like lipase in the intestine, acting on fats; some playing vital roles in tissue building and repair; some guiding, in a step-by-step process, the degradation of foodstuffs, ending with the elimination of carbon dioxide, water, and simple nitrogenous substances like urea. But all the enzymes have one property in common: they are catalysts, differing from inorganic catalysts by being synthesized within the various cells of the body.

The enzymes have another property in common: they are proteins. A number of them have been isolated, and in each case the chemist reports that in their chemical properties they belong to the proteins. But these elegant researches immediately raise a profound problem, which can be put in some such form as this: We know casein (in milk) to be a protein; we know albumin and globulin (in the white of egg) to be proteins; we know pepsin (in the stomach) and trypsin (in the pancreas) to be proteins; yet the pepsin and trypsin are enzymes, and casein, albumin, and globulin are not. Why not? What differences in the internal structure of a protein molecule are necessary to make one protein act merely as a food (casein, albumin, or globulin), and another as an enzyme (pepsin or trypsin)? We have as yet no answer to this question.

And while on this phase of protein chemistry, we may extend our observations to neighboring fields. Insulin, the hormone a lack of which gives rise to diabetes; several viruses associated with disease; and, in all probability, even the very gene itself have been shown to be proteins or substances closely associated with proteins. Again, what configuration of a protein is necessary for it to exhibit the properties of a hormone, a virus, or a gene?

Some of the substances just described

are proteins attached to compounds less complicated than the proteins. For example, the virus and the gene (or, more strictly, the chromosome) have their protein molecules attached to nucleic acid, a nitrogenous substance quite unlike a protein and much simpler in chemical structure, though complex by comparison with the structure of table salt (sodium chloride). Also, the enzymes taking part in the oxidation of foodstuffs in the body are proteins joined to several organic molecules not proteins. These "several organic molecules," in part, are identical in chemical make-up with some vitamins; from which it follows that some vitamins, at least, are needed to build (that is, synthesize) certain enzymes.

A VITAMIN has been defined as a necessary food for the body in the same sense that fats, proteins, carbohydrates, mineral salts, water, and oxygen are necessary foods. But there is one obvious difference between vitamins and our common foods: the former are utilized by the body in minute quantities. This fact has puzzled biochemists for a long time, but the answer is apparently at hand. If, as has been proved in several instances, the vitamin molecule is part of the larger enzyme molecule, since enzymes are able to perform their functions in minute concentrations, it follows that vitamins would be needed in doses equally dilute.

A lack of vitamins may give rise to diseases which have been made well known by the advertising fraternity—beriberi, pellagra, rickets—and we are told that vitamins are needed (in our diet, we would add, rather than as pills) to prevent these diseases. While this is true, the biochemist is more particularly interested in discovering the precise function of these vitamins when present in an organism which appears "normal." We have already more than hinted at this function; now we shall be more specific.

Two of the many enzymes which play dominant roles in the metabolism of the body are known as coenzyme I and coenzyme II. Both have been isolated and their chemical structure sharply defined. Part of their molecular structure —in addition, of course, to their association with protein—includes nicotinic acid. This same nicotinic acid is one of a group of vitamins belonging to the vitamin B complex, and its absence in the body gives rise to the disease known as pellagra. These observations lead to the following conception: Nicotinic acid is needed by the normal organism for the syntheses of coenzymes I and II, which, in turn, are needed for the functional activities of the organism. If nicotinic acid is absent from the diet, coenzymes I and II cannot be synthesized, with the consequent disturbance of certain bodily functions which show themselves, among other things, in the clinical form of pellagra.

Here we have a clear-cut example of how intimate is the connection between a vitamin (nicotinic acid) and an enzyme (coenzyme I or coenzyme II). And this example is but one of several such relationships.

CLOSELY related to the vitamins and the enzymes are the hormones, substances which are discharged into the blood by certain glands and which thereby influence the activity of certain organs. Many hormones have been isolated, and in a number of cases their chemical constitution has been established. Some, like thyroxine and epinephrine (adrenaline), are relatively simple substances; others, like the sex hormones, are more complicated; still others, like insulin, and hormones in the pituitary, are proteins, which makes them the most complicated of all.

As in the case of the vitamins, the absence of one of the hormones gives rise to a disease which is usually characteristic. Thus, a deficiency in the amount of insulin results in diabetes. But an overproduction of a hormone may be equally bad; for example, too much thyroxine (in the thyroid gland) may be the cause of popeyes in exophthalmic goiter.

Again, as in the case of vitamins, we ask ourselves the question: Precisely what role do hormones play when they are present in the normal organism? To repeat, as the definitions tell us, that they "influence" or "excite" organs to activity, leaves us with a feeling of vagueness as to just what is meant.

For at least two of the hormones a much more satisfactory answer has been suggested by Cori, the talented biochemist at Washington University in St. Louis; and it is quite possible that what is true of these hormones is true of others. To appreciate Cori's contribution we must make a few remarks on the subject of carbohydrate metabolism—changes which carbohydrates undergo in the body.

Normally, the amount of sugar (glucose) in the blood remains remarkably constant, despite variations in the intake of carbohydrate. If, however, the production of insulin is deficient, the amount of glucose in the blood increases, glucose appears in the urine, and diabetes results.

This view of diabetes, centering on the production of insulin and following the discovery of the hormone by Banting, has been modified as a result of the researches of Houssay, the foremost physiologist in Argentina. Houssay showed that one of the hormones of the pituitary—we shall call it the diabetogenic hormone—behaved as if it antagonized the action of insulin; whereas insulin tends to prevent the sugar in the blood from increasing, the diabetogenic hormone acts in the contrary direction: it causes an increase of the sugar.

Under normal conditions we have a "balance of forces," the insulin tending to depress the amount of glucose in the blood and the diabetogenic hormone tending to increase it. In abnormal conditions, as in diabetes, where the amount of insulin is small, the diabetogenic hormone from the pituitary runs riot, resulting in the overproduction of glucose in the blood and in the urine.

This balance of forces in the normal organism is of profound significance. For every plus in one direction we usually find a minus in the other. On this basis it might have been fairly easy to predict that if a substance like insulin tends to depress the sugar in the blood, there must be some countersubstance which tends to increase it. Pathological conditions arise when one or the other of these hormones varies in amounts other than normal.

One is tempted to apply this principle, this balance of forces, to the cure or prevention of a number of diseases. For example, in cancer, cells grow at an abnormal rate. Purely as a hypothesis, we may assume that normal growth is regulated by two factors, one in the plus direction and the other in the minus. If we assume, for example, that the factor in the minus direction disappears or becomes less potent, what is there to prevent cell growth from running riot? Obviously, this is an oversimplification of a complicated problem.

To return to Cori's work: When carbohydrates (or, for that matter, fats or proteins) are oxidized in the body to carbon dioxide and water, the change is not an instantaneous one but a step-by-step process. Between carbohydrate and carbon dioxide and water there are many stages, some known, some still unknown. Each link or stage or step is directly controlled by some enzyme or enzyme system. It follows, therefore, that if for some reason

the enzyme system at any one stage is not functioning properly, the disturbance is not confined locally but influences all subsequent chains in the step-by-step degradation. The break in one link in the chain is enough to cause a catastrophe.

One such link in carbohydrate metabolism is reached when glucose combines with phosphoric acid to form glucose-6-phosphoric acid. This combination is brought about by a specific enzyme called hexokinase. Cori now finds that the activity of hexokinase is, in turn, under the control of two hormones, one being insulin and the other a hormone derived from the pituitary (which we shall call the diabetogenic hormone). The latter impedes the action of hexokinase, whereas the insulin prevents the diabetogenic hormone from doing mischief. The insulin, in other words, is antagonistic to the diabetogenic hormone. Normally, both play their role by properly balancing their forces. But abnormally, in what we call pathological cases, this balance is upset.

If, for example, the insulin no longer functions effectively—owing to a diseased pancreas and the consequent decrease in the production of the hormone—the diabetogenic hormone is no longer controlled by an inhibitory force. If such is the case, this pituitary hormone prevents the enzyme, hexokinase, from acting; this, in turn, prevents glucose from combining with phosphoric acid at one stage on its road to ultimate oxidation and elimination. What is the result? The glucose, derived from the food that we eat, is prevented from going further and being used up; it, therefore, accumulates and floods the blood and urine with excessive quantities, bringing in its train the clinical symptoms identified with diabetes.

But from the standpoint of this discussion, the arresting observation is that the hormones, in turn, control enzymic activity. This is quite a change from the statement that hormones "excite various organs to activity." We are now told very specifically that hormones regulate the activity of enzymes and that the latter are more directly responsible for the various changes that much of the food material undergoes in the body.

We can now gather up our separate threads to present a unified picture. At the very heart of the activities we call life are the enzymes. To manufacture many of them, vitamins, among other things, are necessary; and to regulate the activities of some if not all enzymes, hormones are necessary. These are not three separate entities, each going its own way, but all three are intimately related to one another, with a cooperative plan so delicately handled that a failure of any one of them to act immediately upsets the dynamic equilibrium of the body as a whole, resulting in pathological symptoms, or disease.

As a postscript, the fruitfulness of the ideas discussed here can be further illustrated by another, and striking, example.

Enzyme systems are common to all things that have life—from the highest to the lowest; from the complex organism known as man to such lowly forms as bacteria. As is well known, some of the worst diseases to which man is subject are caused by certain types of bacteria—the pathogenic variety. Such bacteria may invade our bodies, grow and multiply, and cause endless havoc. How can we stop their growth and multiplication and so prevent disease and possible death? At various times different methods have been used with varying success. But let us now apply the principle involving enzyme systems. What if we disturb some enzyme system in the bodies of these bacteria and so prevent them from multiplying? What

if we break one link in the chain of reactions that go on in bacteria as they do in human beings? If we succeed, we may bring death to the bacteria rather than have the bacteria bring death to their unwilling host.

This is precisely what we believe the sulfa drugs, and probably penicillin, do to certain types of pathogenic bacteria; the sulfanilamide disturbs a link in the chain of enzyme reactions in the bacteria, with the result that the bacteria die—and man continues to live.

A miraculous result, some cry; but some also ask whether this is merely the survival of the lesser of two evils.

ECLIPSE

Again an age-old miracle occurs,
Known to the ancients even as to us:
An event to which the Odyssey refers,
And freely mentioned by Herodotus.
Again man holds before his finite eyes
The smoke-stained glass, to peer into the sun,
And sees a pearl corona; darkened skies
Design a preface to oblivion.

What are the centuries to sun or moon,
Whose courses cross with measured interval?
Now midnight interviews the crystal noon,
As scientists record the miracle;
Here Science charts the moon's acceleration,
While man is lost in blinded admiration!

MAE WINKLER GOODMAN

THE SELECTION AND TRAINING OF FUTURE SCIENTISTS

By PAUL F. BRANDWEIN

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> Our civilization is a race between education and catastrophe.—
> H. G. Wells.

WORLD events justify special concern with the education of future scientists. Such an education should start early, should be continuous, and should assure us a constant flow of science-talented youth. The teacher in graduate school grumbles about undergraduate training; the college teacher grumbles about training in the high schools. The link between undergraduate and graduate school is easily discernible; that between high school and college is obscure. Nonetheless, it is real; it is important and has played a significant role in the past in the training of our scientists. Dr. Bush's provocative report *Science, the Endless Frontier* clearly recognizes the part high schools must play in such training.

It is my purpose here to describe the preliminary work done in the training of "future" scientists in the Forest Hills High School, which accepts students with varied gifts, opportunities, and economic background. This so-called Training Method, together with other devices, may be helpful in predicting and selecting science-talented youth in high schools. The fact that this article will concern itself with one high-school method useful in discovering, encouraging, and training science-talented students should not lead us to think that science work begins in high school. Science programs are beginning to extend into the elementary school. Soon, the gods of the budget willing, our citizens will have a closely knit science education during the 12 years of their public-school training. The effects on science and scientists of such a program are not difficult to envisage.

At this time, however, the science-talented student is not a clearly defined personality. Does such an individual exist? Or are we thinking merely of a student who combines high intelligence and an ability to work with an originality and initiative which sets him apart only in degree—not in kind—from his fellows? In other words, there is talent in art, talent in music. Is there also a special talent in science? While this question is exceedingly interesting and important, we need not concern ourselves with it here.

This article will deal with those students whose work in science is such that they will perhaps increase its techniques and enlarge its boundaries—in short, with the future research scientist. The phrase "future research scientist" does not imply final judgment; it is a tentative, still unreliable, designation of a student who may distinguish himself in science.

LET us follow an entering freshman class of approximately 350 students at the Forest Hills High School. All of them take a course in general science. This seeks to convey understanding of the common phenomena in their environment, the problems of life and living toward which science contributes; namely, use of natural resources, use of energy for doing the world's work, getting along with other men, and the conquest of disease. The first step in selecting and training the embryo scientist occurs here.

Careful scrutiny is exercised. The progress of students who show any signs of

distinguishing themselves in science is noted during the first few months. Attention is given not only to those whose grades are high, but also to those whose grades are low but who show an ability to work with their hands—those who may have a "hobby" interest in science. These first distinctions are vague, and we would have them so. For our purpose is to give each student the environment to make the utmost use of his gifts and opportunities.

At the end of the first term, those who have shown an interest as well as an ability to work in science are given an opportunity to work in our laboratories during their free periods before, during, and after school. Those who are interested generally ask for this opportunity. Those who are not interested but who have ability—as shown by their work in class—are invited to work. They are permitted to select their field of work or they may be guided into a suitable field. Some of the activities they may choose include the following:

1. Preparing teaching materials in chemistry, physics, or biology.
2. Maintaining a large school museum of a wide variety of living and preserved specimens.
3. Assisting a specific science teacher in his field of special interest.
4. Maintaining a vivarium of forms particularly useful in biological work. Thus students learn to maintain insects and mammals as well as cultures of the common protozoa and algae.
5. Science work in a variety of activities such as *The Science Journal* and the Chemistry, Physics, Biology, or Engineering clubs. They may help prepare broadcasts for the Science Broadcast Club or draw and make models in the Bio-Arts Club. The Weather Bureau, the Agassiz Nature Club, the Museum Curators, the Science Projects, the Cancer Committee, the Youth Council on Health, and the Laboratory Technicians' Club offer other opportunities.

This club program is broad, purposely so in order to attract and hold those interested in science.

During the second term this program of guidance continues. The result is that many good students enter into so-called extracurricular science work. It is our belief that this work is not extracurricular but should and must be an integral part of school work in any science training program. Our students go on to a second year of science work.(This is, for the vast majority of students, a course in biology.) There are many factors responsible for this. Two not inconsiderable ones are the sympathetic viewpoint toward science of the administrative officers and the guidance department of the school and the favored economic position, in general, of the student body. But we also feel that the wide program of science activity offered is in itself a factor.

In the second year of science, selection of students for special science training begins. Toward the end of the first term of biology, students are selected, on the basis of I.Q., reading score, and grades in the first three terms of science, to enter a so-called Biology Honor class or classes, depending on the number available. Out of 350 entering students, some 40 to 60 find themselves in this group. Those students whose programming difficulties make it impossible for them to enter the class are nevertheless given opportunities for similar work.

Generally, students of the highest caliber are found in this class. Among them are the students who are in the first quartile of their graduating class. Regularly, the first 10 in scholastic standing are to be found in this honor class.

Several purposes are served by having these students in one class. They are given a different course of greater difficulty, of more advanced material. They are capable of a high appreciation of scientific method and its social implications. They are given work which stimulates them to develop high efficiency in the laboratory and field. More important than these, perhaps, is that considerable time is spent in personal guidance, so that the opportu-

nities and advantages of entering science are opened to them. The net result is an important preliminary step. Every student from this class takes a third year of science, and approximately 96 percent of them take a fourth year, continuing with physics and then chemistry. Approximately 40 percent of these will take a fifth year of science.

It should not be assumed that these are the only students who enter upon further work in science. Our responsibilities in science education are twofold:

1. We are responsible for a program of general education. This attempts to give those who will not become experts in science the basic understandings, skills, and appreciations that will enable them to cooperate closely with those who will become, or are, experts. This program must also, we feel, have special and practical application to the problems of living, so that the student sees scientific methods at work in solving his personal and community problems.

2. We are responsible for a program of special education. This attempts to select and give special training to those who may become our scientists of the future.

Thus, while practically 70 to 75 percent of our student body of 2,800 are taking science work each term (many of them elect 4 years of science), approximately 200 to 240 students (around 40 to 60 are selected each term after the second year) are in this special science program.

But other aspects of the education of these special science students are emphasized. They are urged to get wide training in mathematics, social studies, music, art, and manual skills. The relation of the scientist to his socioeconomic milieu, the mirroring effect of science on society, is stressed at every point.

Thus, of 350 entering students, approximately 40 to 60 will enter the honor classification and become special science students. Are we justified in making this selection? We have permitted students with scholastic averages of 80 and 85 to enter this classification and given them the same training. The student with a special hobby such as radio or the collection of insects is also selected, regardless of his grades. Whether we are justified depends on the data we will gather from the future achievements of these students in collegiate and postcollegiate work. Every student is given the opportunity to show his ability and avail himself of special training, but we have found that those who have an average grade of 90 or over are able to take the best advantage of the type of training to be described below.

The students selected each term are given the opportunity to—

1. Engage in some "original" research work on the high-school level. Each student is under the close guidance and observation of a teacher. (This work will be described briefly later.)

2. Learn the expert use and operation of laboratory equipment of all types (analytical balance, microscope, electric oven, autoclave, etc.).

3. Learn laboratory techniques (histological, bacteriological, techniques of analytical chemistry, work with glass, etc.).

4. Gain special skills in shopwork, including handling of common materials, wood, metal, etc.

5. Read representative college texts in biology, physics, and chemistry.

6. Take adequate training in mathematics.

7. Prepare exhibits of their work for demonstration before other students, at science fairs, or local exhibitions.

8. Prepare reports of their work (in their senior year) in writing for the school science journal or for other journals.

9. Engage in seminar activity at regular meetings of the Forest Hills High School Science and Mathematics Honor Society. Students who have shown competence in science are eligible for election to the society. Students who offer the best reports of their work will in turn be invited to submit their work at a Biology Congress sponsored by the New York Association of Biology Teachers or to exhibit it at the Science Fair sponsored by the Federation of Science Teacher Associations of New York.

10. Engage in the Annual Science Talent Search of the Westinghouse Educational Foundation.

This is the basis for what may be called the *Training Method* for selecting science-talented students as opposed to the *Testing Method.* Reliance is placed on the observation of these students at work rather than on any given battery of tests. This method is in its preliminary stages of development, but it is described here because of its relationship to the problem of staffing our science laboratories.

In a good many of these activities, the actual scientist can see a picture of the scientist-in-embryo. And this is especially true of the research activity engaged in by these youngsters. Here the student faces a problem he has never faced before. No solution is available in textbooks, and it may take two or more years of work to reach even a tentative conclusion. For instance, do zygospores of *Rhizopus nigricans* germinate? Many textbooks figure such germination. Several of our students find no such evidence on the basis of fine investigation. They find their conclusions supported by authorities in the field. How long does digestion take in the food vacuoles of different protozoa? Why does *Chaos chaos* appear to have only a regional distribution? What factors influence sporting in species of *Coleus*? What effect does the gas produced by *Tribolium confusum* have on other insects? Other work includes a study of the embryology of *Physa*, diapause in Cecropia and other insects, development in vitro of certain plant embryos, studies on a modified method useful in the recovery of silver in photography, the structure of soils in the vicinity of Queens, studies on aberrant electrostatic effects, and others. There is no doubt in my mind as I have observed these young people at work that they go through the operations of the scientist at work.

As they work, they grow. They make the scientist's methods of thought their own —at least in regard to the problem at hand. And perhaps it is not astonishing to find that they feel that these methods hold much promise for use in investigating social as well as natural phenomena. Is this naiveté? Is it just the commonplace rashness or idealism of youth?

This early training and emphasis in the social responsibilities of scientists is of the utmost importance. These boys and girls are still plastic. Perhaps if we train our students in high school and earlier to see themselves as citizens first and specialists later, we shall not have the situation which Dean Harry J. Carman describes in these terms: "In public life we are ruled by scientific ignoramuses, and in the scientific laboratory we have, for the most part, political and social illiterates." The elimination of this state of affairs is a prime objective of our program of general and special education.

In the second year of high school we have, then, 40 to 60 youngsters who are ready and willing to embark on an extended period of work. They are given laboratory space. They must read source material, plan experiments, order materials, construct equipment—in short, over a period of two years, carry out in a small way the methods which serve the scientist. Their teachers are ready to advise them, but advice is forthcoming only when the problem presented is worthy of consideration by the sponsor and is not indicative of laziness, poor thinking, or poor working method. In the case of the latter, the student is given whatever guidance is advisable and turned back to his work.

During the two-year period, many students drop out of the work. Some find that athletics and social events are more important to them than science; others are not fitted—through lack of even the simplest manual skills—to carry on the work; still others lack originality. A few are lacking in honesty or a sense of responsibility and are advised by the sponsor to seek other work, but only after failure

has attended many attempts to produce desirable changes in attitude by methods at our disposal. And, of course, there are those who cannot learn to work with others. Again, we have never released any of these until attempts have been made to make desirable changes, and in some instances we have retained these youngsters till the bitter end—especially when their basic qualities have warranted it.

In any event, by the beginning of the latter half of the senior year we have perhaps 10 boys and girls who have participated in most of the 10 activities previously listed.

From these 10 senior special science students, come the 3 or 4 seniors who, we believe, may be scientists of the future. These 3 or 4 are given a sort of apprentice training in their senior year. They are placed in industrial, college, or research laboratories to assist scientists at work. For this we are grateful to the many individuals in college and industrial laboratories who have given generously of their time and energy. These students distinguish themselves by winning extensive honors in the various activities sponsored by different organizations.

Forest Hills High School is now five years old. Since February 1945, when the first class which had been with us 4 years was graduated, this program has yielded 13 students whom we consider to have promise as research scientists, to have the ability necessary to contribute to science. But approximately 200 students have been given similar training. While we feel that these 13 are of the caliber to make research scientists, we think that these 200 may also enter the scientific field and achieve a measure of success. We do not predict the same kind of success for them as for the 13 but we shall check our predictions against the actuality.

These 13 have won a good many honors. Seven of them placed in the Westinghouse Science Talent Search, 3 among the first 40 finalists; 3 have won the Gold Award for work presented at the Biology Congress in New York. They graduated in the top 20 of their respective classes.

A rating scale is in the process of being developed which will be refined in further testing. This scale has considerable attractiveness from our point of view. The items selected for rating are scholarship (as determined by scholastic average), manual skill, originality, responsibility, honesty, capacity for work, and ability to work with others. We are also developing "paper-and-pencil" tests that seem to be promising. With these, we may be able to predict success in science.

On the basis of our preliminary observations, it is obvious that high I.Q. alone, or high scholastic average, does not guarantee success in science. Our preliminary observations indicate an approximate lower limit in I.Q. of 125–130 (Otis) for the achievements (honors) which these 13 accomplish.

Also, it is our very tentative view that the high-school hobbyist in science (the collector of insects, etc.) who does not have wide general ability may make a good technician but may not make research contributions of high caliber. This conclusion, of course, depends on a close check against the future activities of a good number of hobbyists and a good number of our well-rounded students. Five to 10 years from now we shall be able to make a more reliable statement.

We may draw a very rough sketch of certain characteristics of this "future" scientist. It must be emphasized that this is subject to change depending on further observations. He has an I.Q. whose lower limit is about 125–130, an approximate reading score of 15–16 (9), and a remarkable capacity to work and learn. He is within the upper 10 of his

graduating class. He distinguishes himself in the caliber of his work by winning honors. He has broad general ability. His scholarship shows few weak spots; he is competent in mathematics and languages as well as in science. Usually, he shows high manual skill and inventiveness.

He may be of good or poor economic background and of native or foreign extraction. In general, he shows high social consciousness and is ready to see the relationship of science to society. He believes that the force of science can be used for the good of society. He is good material to turn into the scientist or any variant thereof. But like any embryo, he needs nurture. Shall we waste him?

Is he poor? Then he may not go to college. If he does go, will he be lost in the shuffling and reshuffling of thousands of college freshmen? Or will there be close cooperation between college and high school to make the most of his ability? Or should he be treated like any other student—left to take his chances, so to speak?

We feel that there should be a comprehensive program throughout the United States that will stimulate and maintain the best science teaching in the high schools as well as in the colleges. This program would need funds, but it would also need highly improved teacher-training facilities. There should be a carefully planned program (a national foundation, perhaps) to select science-talented students and subsidize their training. Again this calls for funds, but it also calls for the development of selecting devices. The experiences of the Westinghouse Science Talent Search, of special high schools (such as the Bronx High School of Science in New York), as well as the experience of teachers throughout the country, need to be examined.

It is urgent that science look to its sources. These are: first, a steady flow of the highest type of young men and women; second, the facilities to train them. And, most important, we need—desperately—the teachers who have the training to develop the future scientists of the United States. Science must look to the development of those who are its mainspring and hope.

SEEING AND BELIEVING*

By C. JUDSON HERRICK

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SEEING, they say, is believing. This is true, no doubt, but sometimes what we believe we see does not square with what is really there to be seen. Every such experience is a challenging problem, a problem that science is expected to solve because we have to make our livings in the world as it is, not as it may seem to be. If I am seated in a stationary railway coach looking at a train passing on the adjacent track, I may believe that I am moving very fast. The speed of the relative movement of the trains may be correctly judged, but my orientation with reference to it is wrong.

In science, just as in common life, the true meanings of things are expressed as relationships, and the meaning of a thing is not an impersonal abstraction. It means something to somebody, and this relationship of the observer to the thing sensed is an ineluctable component of every experience, whether or no the individual has any awareness of the experience.

The dogma enshrined in scientific tradition that scientific truth is somehow detachable from the experience that validates that truth and employs it in the search for the meanings of things must be thrown into the discard along with the other discredited mythologies. All data of science and all generalizations derived from these data are defined relativistically from the standpoint of the observers upon whose experiences they depend. This relationship to the observer is inevitable; we cannot escape it, for it inheres in the very nature of experience. What we must try to do is to enlarge our experience so as to include all the relevant data and so avoid the pitfalls which entrap us when essential components of the situation are ignored.

Let me illustrate by a few examples from my own experience.

My brother Clarence and I, on one of our scientific excursions, went from Cincinnati to the hill country of eastern Kentucky. On the return we took a night boat down the Ohio River, and on awakening in the morning I was annoyed to find myself disoriented. The feeling that we were moving eastward and upstream was irresistible, and it was unaffected by any rationalization about it. The fixed idea was impervious to the knowledge that we were actually going in the opposite direction. This has been a common experience with me, and I resolved to try to analyze it and see what actually happened in my mental process when the false orientation was corrected. As we approached Cincinnati I seated myself comfortably at the bow of the boat and intently searched the shores for the first glimpse of a familiar landmark. Unfortunately, my attention was distracted for a moment, and upon raising my eyes there was the silhouette of a well-known house against the sky on the right bank. It was on the summit of Mt. Lookout just where it should be, and the apparent direction of our movement was instantaneously reversed without effort and without the slightest participation of conscious experience. This experiment has several times been repeated, and I have never been able to observe anything that would help to explain how the reorientation was accomplished.

Another problem of orientation has repeatedly been encountered in my subsequent travels. How do we judge distances

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of objects in the field of view? Or is it a judgment? Perhaps it is a direct perception, or it may be that neither perception nor sensation, as conventionally defined in terms of awareness, are involved but some quite unconscious sort of nervous adjustment.

On my second visit to New Mexico, I arrived at Socorro after midnight, was taken to the home of my brother Will, who was a U. S. deputy land and mineral surveyor, and immediately went to bed. In the morning my first glimpse of our surroundings was a narrow vista seen through the window opposite to which I was seated at breakfast. The field of view was almost completely filled by a roughly conical pile of rock pointing upward into the sky. I was perfectly familiar, from previous experience, with the clarity of that atmosphere and the difference between the aerial perspective there and in my home environment in Ohio. I speculated: Is this perhaps a mine dump a few hundred yards away or a big mountain on the far side of the mesa? Without asking questions, I tried my best to estimate its distance. There was neither foreground nor background, and I could find no way to determine how big or how far away it was, though every detail was brilliantly clear. Not until I was outside, with a wide sweep of horizon in view, did I recognize that it was the peak of Socorro Mountain five miles away, a peak which I had seen from a different angle on a previous visit.

This readjustment of my visual habits to a different atmospheric condition had a curious sequel. After a few weeks in the mesa country, most of the time in travel by wagon, upon my return to Ohio the recent readjustment of visual habits persisted. My house in Granville was on College Hill, looking out across the wide flat valley of Raccoon Creek to a backdrop of lower hills on the opposite side. Rising from the floor of the valley at the foot of my own hill was a conical upthrust known appropriately as Sugar Loaf. On the morning after reaching home I looked out across the valley and over the bare summit of Sugar Loaf, where there was a group of people. To my amazement, they were all giants!

The explanation came to mind at once, but the illusion persisted. The men were about a quarter of a mile away, and the intervening haze dimmed their outlines more than twice that distance would have done in the clearer air of New Mexico. The lines of the visual angle projected upon my retina by each man were, accordingly, extended outward to a greater distance, and his stature rose to gigantic proportions. I was perfectly familiar with every foot of that topography, and the actual distances were known. The apparent size of these people was certainly not given to me by any process of rational judgment; indeed, I knew that it was irrational. It is as if the sensory experience was presented to consciousness as a finished product completely fabricated below the level of awareness.

A similar problem confronted me on my first visit to the Grand Canyon. Again my arrival was at a late hour of a very dark night, and I retired at once in one of the cabins provided by the hotel. In the early morning I dressed hastily, keyed up to a high pitch of excitement in anticipation of the big thrill. A few steps from the door took me to the parapet and I looked down.

Yes, here was a chasm; but how big and how deep was it? Was this the Grand Canyon, a mile deep, and was the opposite line of cliffs the North Rim, thirteen miles away? Or was I looking into an insignificant lateral tributary with dimensions measured in hundreds of yards instead of miles? To my chagrin I could not answer this question. It was like looking at a photograph of a gully in a clay bank which might be an inch deep or a hundred feet. The whole circle of the horizon was spread

before me, but there was not a landmark that gave a clue for estimating distances or dimensions. Instead of the anticipated thrill of amazement I got only puzzlement, distrust of my own perceptive acumen, and shattered complacency.

If I had turned away and left the Canyon at that moment, it would have been to join the hosts of little men who look at big things without seeing them, typified by the English tourist who had registered at that same hotel a short time before. He was an inveterate globe-trotter in the conventional British tradition and costumed for the part. Stepping to the parapet, he looked out for perhaps ten minutes and then returned to the desk with the query, "When is the next train out?"

"In twelve minutes."

"Righto. You may send my luggage down to the station."

"Why, aren't you going to stay to see the Canyon?"

"Aw, I've seen it."

Not until I had walked down to the river and up the six thousand feet of tortuous trail to the opposite North Rim and camped for several weeks around and within its castellated walls did I feel that I had begun to grasp the stupendous magnitude of the thing and to appreciate its glory. After several such visits I feel that now I have seen it, and this experience has made me a bigger and so (I trust) a more humble man.

THESE incidents suggest three topics that men of science usually shy away from as of philosophic rather than scientific interest; but that is a mistaken attitude, for science is vitally concerned with the relations between appearance and reality, and so we must examine critically the nature of the evidence upon which we must base our judgments. First, we must have some sort of working hypothesis about the relations existing between our knowledge of the objective world and the use that we make of this knowledge in reasoning about it; that is, between what we may call, for short, objective, or sensorimotor, experience that is projected outward and those subjective experiences that have no such external reference. We must inquire into the nature of experience itself as we employ it in scientific work and try to arrive at what I call a natural history of experience.[1] In the second place, our ideas of the nature of scientific evidence and explanation need to be clarified. Third, how do the conclusions reached on these questions bear upon the place of science in human economy and practical problems of adjustment?

The first of these topics is basic for science. Illusions of sense, for instance, must be analyzed, not only in terms of objective realities, but also in accordance with what we can find out about both the cerebral mechanisms involved and the accompanying subjective mental processes. All these things really exist, and we cannot understand the illusion without taking them all into account. The discovery of these relationships is a scientific problem which will never be resolved by metaphysical dialectic.

It has long been recognized that introspection can give us only the end result of a process, a large part of which goes on without any awareness of it. So the subconscious has invaded the domain of introspective psychology; but, since these underlying physiological processes are inadequately known, a way of escape has been sought by personifying them and endowing these fictitious daemons with capacities which, so far as we know, are exercised only by rational persons. These subterfuges must be replaced by more exact knowledge of what actually goes on in the body preceding and during the conscious process as we know it introspectively.

Our ignorance regarding the bodily concomitants of the mental life has in other quarters led to the choice of a different, indeed an opposite, form of escapism. Acting, I suppose, on the principle that what you do not know will not hurt you, all introspective experience is ignored, ruled out as an epiphenomenon of no significance in the control of behavior—this in the face of the evident fact that every purposeful act that we perform is motivated and directed by affective and rational mental acts.

The dilemma can be resolved only by frank acknowledgment of the well-established fact that our conscious experience and the subconscious (that is, actually unconscious) bodily activities that underlie this experience and generate it operate in accordance with laws which, in the present state of our knowledge, are not directly commensurable. No refinements of introspective skill in observation and analysis can be expected to reveal to direct awareness the mechanism by which this awareness is created, for the laws of thought as revealed introspectively and the laws of operation of material structures as examined extraspectively are unconformable. The converse, of course, is also true. It is a vain hope to anticipate that we shall ever be able to construct a mechanical model of cerebral activity designed in accordance with the laws (known or unknown) of objective mechanics that will give us direct insight into the mechanism of subjective experience.

This is a cardinal principle of traditional metaphysical dualism, but this philosophy unfortunately is incompatible with any truly scientific approach to the body-mind problem. Nature cannot be split apart in this fashion. And, again, to deny the validity and primacy of either objective or subjective experience is a subterfuge that merely obfuscates the problem. Neither traditional idealism nor traditional materialism is scientifically tenable. The only way open to the naturalist is to recognize the disparity of introspective and extraspective experience and then try by some indirection to discover the still unknown laws in accordance with which their obvious interrelationships are explicable. This again is an approachable problem.

THE second topic suggested here raises these questions: What do we mean in science by explanation? And to what kind of evidence must we appeal in a search for explanation?

Everyone agrees that science should describe all that is found in nature and how it operates, but eminent authorities insist that science has no right to ask why things are what they are and why they do what we see them doing. Nothing could be more absurd. The structure of things, how they work, and how they came to be what they are, these are essential steps toward comprehension of their meaning; but we do not grasp the full import of that meaning until we know why they have this structure and why they behave as they do.

In the *Encyclopaedia Britannica* (11th ed., **24,** 403, 1911) we read: "An 'explanation' proves to be simply a restatement of a phenomenon in terms of other phenomena which previously are familiar to the mind, and therefore appear to be better understood."

This definition of scientific explanation which makes it scarcely more than a tautology misses the essential point of it. The restatement of a phenomenon in terms of other familiar things has value for clarification of what it is only because it brings to light additional relationships which before were not apparent. The familiar things are better understood only insofar as their relationships are more completely known.

Here is a rock that looks like a tree trunk. It is not a piece of a tree, of course, because

it is made of mineral, not wood. Chemical analysis identifies the mineral and sets it in its proper niche in our classification of familiar minerals. But this does not explain why it looks like a tree. Grinding and polishing a broken end reveals a pattern of rings like that seen in living trees. Here is an additional relationship that justifies the inference that this rock is a petrified tree. The botanist may find that its structure agrees closely with that of some well-known living species, and so he gives it the same name. He counts the rings and tells us how old this tree was when it fell. He may even give us some reliable data about the climatic changes that occurred during the life of the tree. Now the geologist is called in to examine the terrain and tell us about how many thousand years ago this tree lived. We now have a scientific explanation of our specimen in terms of verifiable relationships, an explanation that is far more than a mere list of other familiar things. We know why this thing that appears to be just a piece of rock looks like a tree.

Darwin described many ingenious devices by which insects are induced to cross-pollinate flowers. Doubtless the bees and the moths do not know why they do this, but there is a reason. The intricate floral structures of orchids and the correlated behavior of the insects are quite meaningless apart from the end to be achieved. This is why they exist. The organic realm is shot through with similar adaptive adjustments. The primary meaning of all adaptations is survival; and beyond this are countless others, all of which may be subsumed under the term satisfaction. The fangs of the tiger are adapted for the satisfaction of hunger, and we men have set up aesthetic and ethical standards of conduct because these give us our deepest satisfactions. Life is not aimless. All vital activities are directed toward some end; it may be food, mates, or exuberant playfulness just for the fun of it. This is what life is for, why we live—to get as much satisfaction out of it as possible.

This hedonistic view of life is not ignoble. It is as broad and as rich as life itself, embracing every vital interest from mere survival value on through the limitless hierarchy of values up to our finest aesthetic and moral aspirations. We cherish these higher virtues because they are good for us and because they make for fuller and richer lives.

In the inorganic realm it is harder to find reasons for things, and this perhaps is why so many scientists take the easy way out and pass the buck to the metaphysicians. We know how a geyser was made and why it operates as it does in accordance with familiar hydrothermal principles. But why is a geyser? And why is the Grand Canyon? The structure and growth of the Rocky Mountains have been well documented; also the correlated volcanic, seismic, orographic, climatic, floral, and faunal changes. The how is fairly well known, but why? The further course of these changes can be projected into the future with some assurance, and this predictability of natural events is one of the greatest triumphs of modern science. We can forecast the time when the Rocky Mountains will be reduced to a peneplain, just as other mountain ranges are known to have been leveled in past geological ages. Tide tables, weather predictions, and all the rest are possible because all natural processes are interrelated in an integral orderly system. The task of science is to discover the laws of this order and to show how to adjust our human affairs in accordance with them. Some of these, like the phases of the moon and the generations of men, come in repetitive cycles, and when the full cycle is known, any observed phase can be set in its place in the cycle and the next phase predicted.

Many natural processes have directive trends that we can discover; otherwise prediction of future changes would be impossible. Insofar as the directions taken by these movements of cosmic events are recognizable, we can begin to understand why particular incidents happen as they do. We get the meaning of the detail, its scientific explanation, only insofar as we learn the place it occupies in the larger pattern of directive movement. These patterns of inorganic directive (as distinguished from random) movement can be recognized all the way down the scale of magnitudes from nebulae to electrons; and, as has been pointed out by Lillie,[2] they have some characteristics in common with those which in organisms we call adaptive.

All this leads us back to our claim that the why of things is the cardinal scientific problem. The meaning of any natural thing or event cannot be fully grasped or explained scientifically until we discover its relations to the other components of the orderly flow of process that we call our cosmos. Not all of these relationships are relevant to any particular inquiry. A critical or pivotal "referent" (to borrow an expression suggested to me by Dr. S. H. Bartley) is selected, deliberately or unwittingly, and others are assembled with reference to it.

If the proximate goal of any movement, organic or inorganic, is recognized, our explanation is satisfactory up to that point. Radium spontaneously disintegrates into atoms of different structure and properties; an incandescent mass may become a planet with people living on it; a continent was desiccated by climatic change, and fishes became amphibians; a man wants to talk with his distant associates, and he invents a telephone. In none of these instances is the course of events random or haphazard. From the standpoint of the observer, there seems to be an end to be achieved and an orderly process directed toward that end. The mechanism of this apparent teleology is intrinsic to the natural system in operation; it is not imposed upon it from without. This is what we mean by saying that it is natural, not a mystic thaumaturgy. Moreover, cosmic events do not run in uniform cycles; the figure is spiral, not circular. Throughout all nature new things emerge, and these too must be explained. This doctrine of emergence also must be kept within the frame of the natural.

FINALLY, how does this naturalistic conception of experience and of the part that experience plays in scientific explanation bear upon our present problems of adjustment to the realities of the world in which we are living?

The naturalist's attitude toward nature and his interest in its mysteries differ from those of the intuitionists, whether these be artists, poets, or philosophers. The naturalist wants to find out, not only what there is, how it came to be, and how it works, but also the why of things. His understanding of the meaning of what he sees is incomplete until he has some comprehension of its *raison d'être*. His sensitivity must be as acute and discriminative as that of the artist, though applied in a quite different realm of creative artistry in a quest for values of different order. And his imagination must be not less vivid and nimble. He gets satisfaction out of his facts, just as the artist does, but for him the full measure of satisfaction is achieved only when the raw facts are set in a frame of relationships that reveals a valid explanation of them.

"Art for art's sake" has been a popular slogan, but the great artists have not practiced it so. Art for life's sake is a truer motto, for the successful artist—painter, poet, or actor—lives his art, and if he cannot make it live in others it is feckless and sterile. So also in the life of science "truth

for truth's sake" is meaningless verbiage. The truth that makes you free is live truth incarnate in human struggle and aspiration. Truth for life's sake gives point and poignancy to our search for the truth about things and the meaning of things.

A reorientation of the scientific outlook is indispensable if science is to meet the obligations thrust upon it by the exigencies of our time. The method of science must infiltrate every human enterprise. We must learn to regulate all our affairs, personal, social, and international, in the light of actual experience, critically evaluated, rather than blindly to follow outworn tradition or appeals to prejudice, greed, and lust for power. Science is not the only avenue to this goal, and we must work for more harmonious collaboration with our colleagues in the so-called humanities.

The general neglect and misunderstanding of the true methods and objectives of science are shockingly prevalent. A recent writer[3] solemnly assures us "that the modern scientist no longer considers that he can explain anything." The same issue of THE SCIENTIFIC MONTHLY that contains this benumbing potion supplies the antidote. Professor Benjamin[4] in an enlightened plea for the humanizing of science remarks, "Seeking for reasons is, in the broad sense of the word, science itself."

Not long ago I published a plea for the recognition and enhancement of human values in all scientific work.[5] These human interests belong in science because there would be no science apart from them, and we work for the advancement of science because it is good for us and because it enlarges the range of human satisfactions. This is the why of all science and in the social sciences this is conspicuously the crucial issue. I asked:

> In what kind of a world do we want to live? This is the most urgent problem of today and tomorrow. We can get what we want within reason if enough of us join our forces. We can exploit other people in our local communities and in the community of nations and acquire wealth and power—at the cost of continued disorder and sooner or later of war—or we can cooperate in competitive enterprises in lawfully ordered ways and win peace and prosperity.

Social comity and harmony imply cooperation and some measure of renunciation of personal and national sovereignty. In current practice we recognize this in the domain of sport. In a boat race the two teams are intensely competitive, but the competition is regulated by rules of the game previously agreed upon. The good sportsman abides by these rules and does his utmost to win in conformity with them. It is a curious anomaly of our culture that in our hours of relaxation, in our recreations and sports, even in our prize fights, we voluntarily cooperate in competition in accordance with the rules, but in the serious affairs of life, in business, politics, and diplomacy, we revert to the laws of the jungle. No advanced and progressive civilization can be built on this foundation. If our humanism cannot emerge from this morass of bestiality, we do not deserve anything better than the turbulence which now obstructs every effort to stabilize our social structure.

To all superficial appearances we are drifting in industry and diplomacy toward perdition, at the mercy of stupid leaders intent only on personal, group, or national advantage, with ruthless disregard of those principles of fair play upon which all real social progress so far achieved has depended. This conflict of intransigent belligerents is not illusion. It is all too real throughout our social structure, from the family circle to what we optimistically call the United Nations. But fortunately this is not the whole of the picture.

Under the surface of the recrudescence of

struggle for supremacy there is a strong undercurrent of genuine humanitarianism which plays the game of life according to different rules and for different objectives. More efficient socialization of the struggle for existence and for a place in the sun is what separates man from brute. Language and all our rational and aesthetic skills are the instruments employed. In the course of this struggle standards of decency, honor, consideration of the legitimate rights of others, and some measure of sacrifice have been slowly—very slowly—so deeply entrenched in human nature that no temporary or local reversions to barbarism and bestiality can long survive. Tyranny, whether on a small scale or large, has within its essential structure the elements of its own destruction. For this we have ample scientific evidence in the history of the evolution of human culture, and equally good evidence that faithful observance of well-conceived standards of honor, justice, and intelligently directed teamwork have actual survival value in our world today. This, too, is not illusion.

Human nature is basically sound, vigorous, enterprising, and decent. However various may be the standards of propriety among different races and peoples, some standards there always are, and in general people conform with the mores of the clan. But these social standards are not immutable, and the immediate task is a discriminating appraisal of past experience, especially of recent history, to learn which of the numberless patterns of social organization and practice that have been tried yield the social stability and opportunities for further advancement that we crave. This problem can be solved only by application of the scientific method, not by wishful thinking.

Our scientific explanations are never completely finished, and the meanings discovered are approximations, not ultimates. But they are the best we have, and past experience is our best guide. More knowledge will point the way to more successful application of this experience, but knowledge is not enough. We must learn to discriminate illusion from fact and then to interpret and utilize these established facts for judicious selection of the objectives that we want to reach and of the methods to be employed for their attainment. And to this knowledge we must add faith, courage, and the will to win what we must have if we hope to survive.

The Apostle John warned, "Judge not according to the appearance." Such a judgment can lead only to despair if Thomas Moore was right,

> This world is all a fleeting show,
> For man's illusion given;

for we have to adjust to this world somehow, and it is up to us to learn how to do it. Shakespeare's appeal,

> Here we wander in illusions;
> Some blessed power deliver us from hence,

was appropriately written in his *Comedy of Errors*. Our deliverance from illusion must be won by our own efforts, and science must point the way.

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PRIVATE FLIGHT TO THE BOSTON MEETING

By GEORGE V. CAESAR

Staten Island, N. Y.

DR. LANGMUIR learned to fly before I did. But if he attended the sixth Boston Meeting of the A.A.A.S. I doubt if he flew there. I think I was the only member who did. This at least seemed plausible when I crawled out of my little cabin plane at Logan Field in that icy gale of Thursday afternoon, December 26.

To begin with, I had no intention of flying up to Boston. I had planned to travel more conventionally—and in boredom—by rail from New York. But the unusually lovely weather on Christmas Day seduced me. The urge to fly, suppressed in winter, boiled to the surface. It seemed likely that the fine weather might hold over the week end. So I phoned the La Guardia Airways Weather Bureau for their opinion. It was less optimistic: a cold front was moving east across New England, marked by intermittent snow squalls and strong winds, due to pass over Boston Thursday morning. Nevertheless, "contact" flying conditions should generally prevail, and Friday should be "clear and visibility unlimited" (CAVU). Saturday was doubtful, Sunday also, but I could get more certain forecasts on Friday. Was I capable of negotiating pretty high winds in landing at Boston on Thursday? I thought so. Later I wasn't so sure.

The next move was to rent a ship. Most private pilots still find it economical to rent their flying time. It was short notice, but I managed to lease a 3-seater cabin plane of 100 h.p. at Hadley Field near New Brunswick, N. J., a safe comfortable little craft cruising at better than 100 mph on about 6 gallons of gas per hour. I retained my rail reservations pending a final decision early Thursday morning.

Christmas evening I pored enjoyably over the two sectional aeronautical charts of the U. S. Coast and Geodetic Survey covering the New York and Boston areas, scale 1:500,000 on the Lambert Projection, in which straight lines represent approximate Great Circle courses. A wealth of detail, all for the benefit of pilots, is engraved on these remarkable charts. I drew a pencil line from Bridgeport, Conn., to Boston, marked off 10-mile intervals, measured the true course by reference to the middle meridian, and added 14° for mean magnetic variation. This gave a sufficiently approximate magnetic course subject to the plane's compass deviation correction. It was unnecessary to plot the course for the 80-mile leg from Hadley to Bridgeport, the north shore of Long Island Sound providing a sufficient guide. From Bridgeport to Boston was 136 miles. As an alternate route, in the event of poor visibility, I proposed to follow the Sound shore further east to the neighborhood of Stonington, where the New Haven railroad bends inland, and guide on it to Providence, R. I., a course from Bridgeport of about 153 miles. The difference of 17 miles might be well worth uncertain groping in low visibility over a compass course. I fly for fun. Also, I want to be an "old pilot."

Thursday morning was misty. But the weather forecast remaining the same, I drove to Hadley from Staten Island, noting cheerfully that the pall of fog and smoke was thinning rapidly under a clear sun and spanking breeze. Not a sign of that front of cold air and the precipitation preceding it

as the warmer, moister air is thrust up. Soon I was skimming over New York City at 3,000 feet, a stiff wind from the left and enough of it on the tail to boost my ground speed to better than 120 mph. Visibility was excellent; I could pinpoint my position; Long Island Sound was indigo blue, very beautiful; I was warm and comfortable, relaxed, happy.

Then with a rapidity almost incredible the weather picture altered. From warm blue skies I was heading squarely into a dark gloomy wall of wind-torn stratus, its frayed bottom well below my level. I throttled back, put the plane's nose down and dove under the curtain, the long sinuous fingers of cloud reaching vainly for me. Here was the front. And the air in and around it was boiling. I eased the shock of the "waves" by slowing down to around 70 mph air speed and steering out over the water where the bumps were not quite so violent. A few minutes more and the visibility shut down in white driving lines of snow—not dangerously down, but I knew it would be wise to land before things could grow worse. The municipal field at Bridgeport was not far ahead near the tip of a long peninsula. Peering out a little anxiously, I soon spotted its huge dark cross, the white hangars, and the little pimple of the control tower, which I kept constantly in sight as I circled in a wide upwind approach. Without benefit of radio, I was waiting for the green light. It came: a bright cheerful wink, and I knew I was cleared to land. That warm air-lines office felt very homelike.

Airport hospitality is genuine and wholehearted. At large commercial fields, nonscheduled flying is a nuisance at best. But I have never failed to get prompt and courteous assistance when I needed it. At Bridgeport I was kept posted for more than an hour on the rapidly changing weather conditions as the front passed eastward. At 1140 hours I took off with Providence and Boston reported CAVU, wind shifting to NW and increasing. Bridgeport still had a solid overcast, but it was lifting and the snow had ceased. I decided to follow my alternate route. Pilotage would be easier and the bumps softer, at least until I had to turn inland.

However, I overlooked the possibility that I might overtake the front while skirting the Sound. And around New London I did catch up with it, snow squalls and all. But I knew that as soon as I could turn northward at Stonington I should be flying steadily into better weather. This was no mean comfort, as at only 500 feet above the rails I cut corners on their long curves over low hills dusting thickly with snow. On and on I flew, the wind a trifle ahead, the snow cold and dry and showing no tendency to stick. My position in the low visibility was uncertain but caused no worry. Somewhere ahead were Providence and CAVU, down below were the guiding rails, the real pilot.

And so they were. At around 20 miles south of Providence I broke abruptly into brilliant winter sunshine, a glorious sight and a glorious feeling, too. There remained only a 65-mile battle with the rising wind.

It was 1330 hours when my wheels touched down on Logan Field. I say "down," advisedly. I had to plant the wheels down with forward stick pressure and plenty of power. No 3-point landing would do in that wind! I dared not let the tail get down. Later I learned that the wind at ground level was 30 mph with gusts up to 50. I literally flew along the runway, only a few pounds of load on the wheels, until close to the control tower where I had something of a lee and could very slowly and carefully turn left toward a hangar storing light planes. I looked desperately for ground help. To climb out without a man at each wing tip was impossible. Help finally arrived, and my faithful but temperamental

steed was tied down securely. That wind was icy. But somehow I felt overheated as I walked to the hangar.

Flying time from Bridgeport via my longer route was 1:50, an average of only about 85 mph. Total time from Hadley was 2:30, for an over-all average of around 93 mph. Gas and oil costs were $3.92. The landing fee at Boston was $1.00; no charge at Bridgeport, as at most other fields.

A taxi took me to my company's branch office, thence to the University Club, where I was very hospitably put up. Walking to the Armory was an ordeal that afternoon, as those of us who arrived Thursday will affirm. The wind was still rising, and I gave thanks that I had landed when I did. After registering I spent the rest of the afternoon at the intensely interesting Science Exhibit, well worth the trip in itself. It was simply too frigid that evening, and the enticement of the Special Meetings insufficient to lure me from the warmth and comforts of the club. I dare say, if truth be told, I wasn't alone in finding excuses to stay indoors.

Next morning, Friday, was beautifully clear and—to me at least—bitterly cold: around 0°F. But the wind, fortunately, was only moderate. Still, the short walk to the Statler was hardly a pleasure. I attended the morning General Technical Session, listening to extremely interesting addresses by Vice Admiral Bowen, Dr. Waterman, and Rear Admiral Parsons, Director of Atomic Defense. Admiral Parsons closed his talk with motion pictures in color of Operation Crossroads. The underwater explosion, in particular, is indescribable. No one who saw those pictures can ever forget them.

Unhappily, I could not wait for the afternoon session. The weather forecast that morning was ominous for Saturday, Sunday, and probably Monday. I had to be back Sunday afternoon at the latest and could not chance it. A pilot, especially a private pilot constrained to fly contact, is a slave to weather. And so, after a quick lunch and another call at our office, I was back at the airport again, not a little apprehensive as to how difficult (if not impossible) it might be to start my engine after a night in zero weather.

My fears were justified. The hangar help, with kindest intentions, had tried to warm up the engine for me but succeeded only in running down the battery actuating the self-starter. There was only one thing left to try: get some willing husky to swing the prop the while I goosed the primer and muttered. The engine came to life eventually, and I was off at 1345 hours after taxiing for about half a mile in front of two huge airliners, who seemed to chase me like two fat geese after a little frog. Probably I annoyed them more than they worried me.

The return trip was uneventful until I neared New York. I flew a long route of about 275 miles, via Worcester, then southwest to intersect the Sound shore at Saybrook. The visibility was wonderful, and I wanted to see as much as I could of New England. From Worcester south I flew at around 500 feet above the hills, both to reduce a moderate head wind and enjoy the thrill of flight at low altitudes. Generally this is to be deprecated, but, sparingly indulged in and with perfect visibility, it is a great luxury. The air was as smooth as the waters of a sheltered lake. I sat as on a magic carpet beautifully responsive to the slightest touch of the controls, skimming in a beeline at 100 mph over lonely farmhouses, hills, valleys, frozen lakes, an occasional little town. One had the curious dreamlike feeling of a free disembodied spirit aloof utterly from all cares and responsibilities of the flesh. It is a spiritual hygiene to be thus alone, a state for which there is a desperate need in modern living. The love of flying and its flaming evangelism stem from this sense of isolation and freedom

which our pioneer ancestors craved, the outstanding characteristic of our race.

As I turned westward along the Sound, the high thin overcast began to lower and thicken. The forecast for Saturday looked correct, and I was glad to be going home. At 1530 I flew across New Haven harbor, the familiar towers of old Yale in the distance. There was but one hour more of daylight and close to 100 miles to go. I pushed the throttle nearly wide open. It would be a close thing, but if need be I could land at Flushing or Staten Island. Still, I ought to make it, in spite of the head wind which was increasing a little. But when I came opposite Stamford the thickening gloom ahead began to breed doubts. In any event I would have to pull up to at least 2,000 feet to fly safely in reduced visibility as I approached New York. But if I did, could I still see the ground? It was not clouds which I was running into, just smoke and light fog. I could sense the top of this "smog" belt overhead. I climbed slowly to 2,000. I was nearly "on top" but dared go no higher. I could only just see the indented shoreline. I would hold this altitude and course for a few minutes. If visibility got no worse I would push on; if it thickened the slightest bit more I would descend and fly back to Bridgeport. But the conditions held—no better but no worse—so I flew steadily on, guiding by that scalloped shoreline as Long Island Sound narrowed toward the East River. Without it I would have been lost instantly. For awhile it was unpleasant flying. I realize now that I would have done well to spend that night at Bridgeport and I am not proud at all of having got away with about 15 minutes of semiblind flying

I was somewhere opposite La Guardia Airport when the visibility suddenly extended. It was not unlike the opening-scene effect of a moving picture, and I never saw one I liked better.

I landed at Hadley at 1630 with a few minutes of daylight to spare, average ground speed about 100 mph. Private air travel is still in the pioneer stage. It has its drawbacks; I have not tried to gloss them over. But to me it is well worth its tribulations. I hope to fly again to another A.A.A.S. Meeting.

Book Reviews

SOCIAL RECONSTRUCTION

Research and Regional Welfare. Robert E. Coker, Ed. vii + 229 pp. $3.00. Univer. of North Carolina Press. Chapel Hill. 1946.

THIS is a volume of 14 papers on the need for, possibilities of, and methods of maintaining research in the interest of regional and national welfare presented as part of the Sesquicentennial Celebration of the University of North Carolina at Chapel Hill in May 1945 by "distinguished leaders in a dozen fields of action and learning." Included also are an address by the Honorable R. Gregg Cherry, Governor of North Carolina, "Research for the Commonwealth;" a foreword by Louis R. Wilson and an introduction by Robert E. Coker, both of the University of North Carolina.

The volume brings together a most fascinating array of facts and figures on the Southern problem as it relates to the national picture, weighed by the best minds in the fields of agriculture, industry, medicine, public health, social science, education, and government, presented at the hour of the South's greatest opportunity. Signposts of potential progress are set; a plan of action is proposed.

"Too long has the South been the dependent colonial province of a financial industrial empire reaching from Boston to Chicago," says President Frank P. Graham, of the University of North Carolina, in calling upon research to find the solution. Wilson Compton, President of Washington State College, adds: "The industrial East has been the Mother Country. The South and West have been the colonies." He cites figures to show that the South is rich in raw materials which are being siphoned off elsewhere for fabrication to be sold back as manufactured necessities.

Wilbur A. Lazier, Director, Southern Research Institute, calls attention to a score of economic and social log jams in the South that research can dislodge and points to the possibility of bringing privately sponsored scientific personnel together for this purpose. And Raymond R. Paty, President, University of Alabama, sees hope: "Today the South is more research-minded than at any other time in history." He believes the problems of the small-town businessman need more attention.

On the subjects of health and humanities, such scholars as Russell M. Wilder, of the Mayo Clinic, James S. Simmons, Office of the Surgeon General, U. S. Army, D. S. Allen, Johns Hopkins University, and Avery Craven, University of Chicago, show what research has done and can do for the South. In his paper, "History and Social Reconstruction," Dr. Craven makes one of his many great contributions. He refers to the traditional culture and gracious living of Southern people, adding: "Their loss will be a nation's loss." He says:

> Happiness is not entirely a matter of things; it is more than prosperity. It has to do with a way of life and a set of values. Traditions cannot be ignored without cost, and the South by merely becoming like the industrial North will not automatically end all her troubles or gain all satisfaction. She might gain something of richness by looking backward as well as ahead.

Other papers are presented as follows: "Research and Industry as a Factor in Southern Development," by Milton H. Fies, Southern Research Institute; "Needs and Opportunitites for Research in Indus-

try," by Reuben B. Robertson, Champion Paper and Fiber Company; "Research in the Fisheries for the Betterment of the South," by Harden F. Taylor, Atlantic Coast Fisheries Company; "Research and the Southern Farmer," by George J. Wilds, Coker's Pedigreed Seed Company; "The Moral Responsibility of Research," by David E. Lilienthal, Tennessee Valley Authority; "Wartime Science Builds for Peace," by Georges F. Doriot, Office of Quartermaster General, U. S. Army.

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STARS IN THEIR COURSES

The Star Atlas and Navigation Encyclopedia. S. S. Rabl. 161 pp. Illus. $5.00. Cornell Maritime Press. New York. 1946.

THIS book, according to the introduction, is intended for the yachtsman with his first boat, the student-navigator, the amateur astronomer, and all who like stars and the sea. Obviously, a text of 161 pages cannot cover completely the practice and science of navigation and nautical astronomy, as the title suggests. It is, however, a volume from which the amateur or student can learn the essentials of piloting and celestial navigation. It is written in everyday language in a breezy style, which along with the double-column pages, wide margins, and leaded type, makes for inviting and comfortable reading.

The illustrations profit by the large format, 8½ by 11 inches. There are a number of full-page illustrations of star charts and the important constellations. The perspective drawings of the celestial sphere and astronomical triangle and the illustrations of latitude observations, circles of equal altitude, and the like are probably as good a picturization of these subjects as can be found in any textbook.

The first chapter deals with mathematics—simple arithmetic, logarithms, elementary trigonometry, and graphics. This last is dealt with at some length; the advantages of this method over old-time methods of taking into account current and wind in laying down a course are clearly explained.

Succeeding chapters deal with the essentials of piloting, the compass, nautical astronomy, old and new navigation methods, position plotting, the sailings, stars and star-finding methods, and, finally, an interesting, if somewhat fanciful, chapter on primitive navigation. The chapter on the use of bearings is accompanied by an illustration of a distance-off projector for bow and beam bearings whereby this most common of piloting problems can be solved without calculation.

The articles on the principles of construction of both Mercator and gnomonic charts are supplemented by several large, well-drawn diagrams, which should be a help to the student in understanding this important subject. The explanations of the line-of-position computations are likewise well illustrated. The article on the principle of the sextant is accompanied by an animated-cartoon type of drawing which can be understood at a glance.

An important part of the book seems to this reviewer to be the chapter dealing with Modern Navigation Methods. The discussion presents simply and clearly (illustrated by actual examples) the pros and cons of the various short-cut methods for solving the astronomical triangle that have been developed in recent years.

The practical navigator who may be a little hazy on nautical astronomy will find much in this text to help him and will immediately note the few inaccurate statements and impractical methods outlined. An example of the former, found under longitude determinations, is that each minute of time makes a difference of 15

miles, which is of course only true at the equator. The author devotes considerable space to the work of the navigator in observing stars throughout the night, whereas stars can only be used with assurance during about one-third of the twilight periods, a relatively short time. The statement that cross bearings are good only when the vessel is "standing still," if followed in practice, would deprive the navigator of one of his most useful methods of piloting.

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VICIOUS CYCLE

India's Population. S. Chandrasekhar 117 pp. $2.00. John Day. New York. 1946.

FOUR hundred million people are a lot of people. An increase of 50,000,000 people in 10 years is a big increase. Yet India's population is now more than 400,000,000, and it increased 50,000,000 during the 10 years preceding World War II. This is an increase in *one* decade of as many people as the total population of Great Britain.

In this informative and easily read book Mr. Chandrasekhar describes the suffering and the extremely low level of existence of one-fifth of the earth's people. Indirectly, the book brings to mind the living conditions of more than half of the world's people who live in Asia but who do not have even as reliable records as India.

This book has many interesting statistical tables and, in a sense, has the backing of such American population authorities as H. P. Fairchild, Frank Lorimer, Frank Notestein, and Warren S. Thompson, who wrote the introduction. It is divided into three parts: (1) Demographic Facts (facts which are shocking); (2) Public Health (of which there is practically none); and (3) Toward a Population Policy (which tries to explain a way out of an almost insolvable problem).

Here are a few of the cruel facts the author gives us. Some 40 percent of the girls married in India are below the age of fifteen. A widow, be she fifteen or fifty, is banned from remarriage. This means that about 12,000,000 women do not participate in active motherhood. Yet on the average, 200,000 mothers die every year during childbirth or from ailments connected with it. The author explains the causal connection between these factors and how the vicious cycle rolls.

The author tells of housing conditions of the great masses of Mother India which may be difficult for the average American to believe are physically possible; for instance, six families—30 human beings—living in one room 12 by 15 feet!

Despite a percentage increase in literacy in India, in 1931 there were 315,000,000 illiterates, and in 1941 the figure rose to 341,000,000. As in so many other cases, education just cannot keep pace with the birth rate. And if it were not for an extremely high death rate the problem would be even more difficult.

India, as almost everyone knows, has a very high birth and a very high death rate, but the official figures do not tell the whole story. When aliowance is made for underregistration of births and deaths, both rates are about three times as high as those of the United States during "normal" years. And as India's population is about three times as large one can imagine the tremendous wastage of human life.

There is one almost unbelievable fact about India's birth and death rates which is suggested but not described in so many words. This fact is: If India's death rate could suddenly be lowered to the level of that of the United States or England,

with her present birth rate India could fill five earths as large as ours and as full as ours is today, in one single century.

In the concluding part of the book, the author shows the crying need for a rational program of population limitation, soil conservation, reclamation of wastelands, irrigation, industrialization, and a more just distribution of wealth.

One newspaper reviewer is quoted on the book jacket: "One of the great illusions of the twentieth century—that India's burden is too many people—is quickly and expertly destroyed by Mr. Chandrasekhar." The author is a keen thinker and a good writer, but he is hardly a magician. To share the wealth equally in India and at the same time reduce the death rate without reducing the birth rate would indeed produce equality—equality in poverty. While India's high death rate certainly needs to be reduced as quickly as possible, there will be little real progress unless the birth rate is reduced even more quickly.

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WILDERNESS SHANGRI-LA

Driftwood Valley. Theodora C. Stanwell-Fletcher. ix + 384 pp. Illus. $4.00. Little, Brown. Boston. 1946.

NEARLY everyone at some time dreams of going into the wilderness and staying there "away from it all." Few do, and fewer still either enjoy it or get anything out of it, except perhaps a book of adventure describing their good or their unpleasant times. Mrs. Stanwell-Fletcher and her husband, the latter recently recovered from a bad accident, chose a spot on the edge of unsurveyed territory in northern British Columbia, more than 200 miles from a railroad and 30 miles from their nearest Indian neighbors.

A cabin was built to their own specifications, with help from Indians and from a Danish prospector who just happened along. The reader lives with the couple from August 1937 through autumn, winter, spring, and the following summer. An occasional visiting Indian dropped in, coming long distances on snowshoes; sometimes they were loquacious, sometimes silent, but always they had an appetite for huge quantities of hot tea. In the summer of 1938 the author's father and mother came for a two-month visit. Once a plane was forced to land on Lake Tetana, on the shore of which the cabin stood, and two dapper city men, surprised to see a white woman in this part of the world, stayed until the plane could take off again.

There were numerous side trips, each of them a major expedition as far as the Stanwell-Fletchers' experience went. They camped in the bitter weather, as cold as 40° or 50° below zero, stopping under a tree and cooking food that sometimes froze before they could eat it. They fished, hunted game, and collected natural-history specimens to be sent to the British Columbia Museum.

Then by snowshoe, dogsled, and plane, they came out to the civilization of St. George after a year and a half of cabin life. After a short vacation, believe it or not, they returned to Tetana and spent another year, this time partly in the high hills hunting mountain goats.

The book is in journal form, and the author has painstakingly set down voluminous details of the life they led and the food they ate, with observations on the birds and beasts that were their close neighbors. Migrations of birds and animals were watched and recorded. The ducks on the lake, a flock of trumpeter swans, the jays about the cabin, are described, and there are some beautiful passages on the timber wolves and their behavior.

It takes continuous work just to keep

alive in this far northern country, and yet in the appendix there is an astonishing list of a whole museum-full of specimens that were collected and either sent back or taken to the museum.

This is a book for leisurely reading, not one to be rushed through, and it will be enjoyed by those who have been in the outcountries, by those who want to go, and by those who fit into neither of these categories. Not the least of its interest lies in the numerous attractive sketches of animal life done by Mr. Stanwell-Fletcher.

WILLIAM M. MANN
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TREES AND VINES BY PLINY

Natural History. Pliny. Vol. IV, Books XII–XVI. Translated by H. Rackham. 556 pp. $2.50. Harvard Univ. Press (Loeb Classical Library). Cambridge. 1945.

THIS latest portion of Pliny to be translated for the Loeb Library is also the last by H. Rackham, late fellow of Christ's College, Cambridge, who died while revising the page proofs. Mr. Rackham's loss will be keenly felt, for he was doing a fine job with Pliny, and there are still six volumes of the *Natural History* to be translated for this series. This fourth volume of Pliny comprises the books on trees and vines and is a self-contained unit. One might say, offhand, that botany could not offer Pliny much scope for the fantastic lore so evident in his treatises on animals, but such a person does not know Pliny. There are, for example, the bush that will turn into stone if taken unawares and the submarine forests of the Red Sea. This volume is primarily, however, a treatise on the economic botany of the Roman world, especially the vine and its delectable products, the olive, and the other fruits beloved by the Romans. As such, it should appeal to all botanists, connoisseurs of wine, and *bon vivants* in general.

JOEL W. HEDGPETH
Game, Fish and Oyster Commission
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FRUIT AND VEGETABLE CROPS

General Horticulture. Thomas J. Talbert. 452 pp. Illus. $4.00. Lea & Febiger. Philadelphia. 1946.

A REWRITTEN and expanded version of *Fruit Crops* (published in 1939), *General Horticulture* is the outgrowth of 30 years of teaching and investigational work by the author at the University of Missouri. It is intended as a beginning undergraduate text for schools and colleges.

Nearly 300 of the 452 pages of the book deal with North-Temperate deciduous fruits, but short chapters have been added on vegetable crops, on beautifying the home grounds, on tropical and subtropical fruits, and other general horticultural topics so as to broaden its scope. Nevertheless, the principal treatment is of the North-Temperate deciduous fruits.

The subject matter is complete so far as fruits are concerned. There are few, if any, important phases which are not covered. The treatment of each of the many topics is of necessity brief, but adequate without becoming involved. If there is any criticism, it lies in a too brief discussion of vegetable crops, which have now become so important in horticulture.

Throughout the presentation, the horticultural point of view is dominant. Practical considerations and the real purpose of horticulture in providing food for mankind and in providing a living for the producer are kept constantly in the forefront. Selected references at the end of each section make it possible to pursue any particular subject in greater detail.

The printing is well done, the type is

sufficiently large for easy reading, the illustrations are clear and well chosen, and the binding is good—in general superior to the make-up of many books that appeared during the war years.

H. B. TUKEY

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AN INTERPRETATION OF WEBER

From Max Weber: Essays in Sociology. Translated, edited, and with an introduction by H. H. Gerth and C. Wright Mills. xi + 490 pp. $5.00. Oxford Univ. Press. New York. 1946.

MAX WEBER (1864–1920) was one of the great scholars of Germany. He

> . . . belonged to a generation of universal scholars, and there are definite sociological conditions for a scholarship of the kind he displayed. One such condition was a gymnasium education, which, in Weber's case, equipped him in such a way that the Indo-Germanic languages were but so many dialects of one linguistic medium. . . . Urbanism, legal history, economics, music, world religions—there is hardly a field which he left untouched. He thus continued the tradition of encyclopedic scholarship of Wundt and Ratzel, of Roscher and Schmoller (pp. 23, 44).

With these qualifications and interests he proceeded "to write social history in the grand manner." During Weber's time this type of wide-ranging scholarship frequently passed for sociology for want of a better definition of the latter term. Actually it was a philosophy of history. It is therefore an error, so far as sociology is concerned, to say, as does the blurb on the book jacket, that "over the last fifteen years [Weber] has become a leading influence in American thought." For American sociology during the past fifteen years has turned away from the philosophy of history and increasingly has taken its cue from the other sciences, to its very great advantage from all points of view.

The above characterization of Max Weber's place in the intellectual world is in no way a detraction from his contribution or from that of his translators. He displayed vast erudition and many a penetrating insight. Among other things, he showed an understanding of the difference between social science and propaganda which many physical and social scientists today seem unable to grasp. Weber was quite explicit on the subject as, for example, in the following passage:

> To take a practical political stand is one thing, and to analyze political structures and party positions is another. When speaking in a political meeting about democracy, one does not hide one's personal standpoints; indeed, to come out clearly and take a stand is one's damned duty. The words one uses in such a meeting are not means of scientific analysis but means of canvassing votes and winning over others. They are not ploughshares to loosen the soil of contemplative thought; they are swords against the enemies: such words are weapons. It would be an outrage, however, to use words in this fashion in a lecture or in the lecture room. If, for instance, "democracy" is under discussion, one considers its various forms, analyzes them in the way they function, determines what results for the conditions of life the one form has as compared with the other. Then one confronts the forms of democracy with non-democratic forms of political order and endeavors to come to a position where the student may find the point from which, in terms of his ultimate ideals, he can take a stand. But the true teacher will beware of imposing from the platform any political position upon the student, whether it is expressed or suggested. . . . One can only demand of the teacher that he have the intellectual integrity to see that it is one thing to state facts, to determine mathematical or logical relations or the internal structure of cultural values, while it is another thing to answer questions of the *value* of culture and its individual contents and the question of how one should act in the cultural community and in political associations. These are quite heterogeneous problems. . . . The task of the teacher is to serve the students with his knowledge and scientific experience and not to imprint upon them his personal political views (pp. 145–46).

The first 74 pages of this book are devoted to a biographical sketch of Max Weber and a brief account of his intellectual orienta-

tions and approach to social phenomena. The remainder of the book consists of translations of selected essays from Weber's work, principally on the subjects of the power structure of society and the subject of religion. There are also chapters on Politics and Science as vocations and a concluding section on Social Structures, mainly of India and China. Twenty-two pages of notes and an Index conclude the volume.

The main purpose of translations is to make available to a larger audience ideas otherwise restricted to a smaller public. From this point of view, if the ideas are of any importance at all, translations are to be welcomed. Especially is this true when the translation is from the German, for, as Mencken once remarked, German is the only language that seems to have been constructed with deliberate malice. This dictum could perhaps be well illustrated from the German of Max Weber. As the translators point out in their preface, his sentences are "Gothic castles;" "mental balconies and watchtowers, as well as bridges and recesses, decorate the main structure." Readers will therefore gladly pardon the translators for having reconstructed with a free hand this heavy and cumbersome architecture. I suppose there will be the usual quarrels as to whether the result in all respects truly represents the original author's meanings. The present reviewer professes no competence to pass on such questions because there often is really no way of determining in "literary" writing of this sort exactly what the author did mean. Although the present text is still in many places unnecessarily long-winded and abstruse, the present volume is doubtless the most readable translation and interpretation of any of Weber's work now to be found in English.

Whether the material translated is worth the effort will again be a matter of opinion with which this review cannot concern itself to any great extent. The distinction of Weber in his time and in the fields in which he worked have been noted above. Insofar as the appearance of the translation at this time represents another of the periodic "discoveries" of some European author, usually after his work is obsolete, and an attempt to stage a revival over his remains, these efforts will be pretty sterile. This is no reflection upon Weber, for, as he himself said, "In science, each of us knows that what he has accomplished will be antiquated in ten, twenty, fifty years." Certainly the same applies to philosophies of history. After the 25 years and more since most of Weber's writing was done, I believe Weber would be the first to admit the applicability of this statement to his own work, especially as far as sociology is concerned. I think I have all due regard for the insights and prodigious labor of men like Pareto and Weber. But I think they had been thoroughly superseded in this country by the time their words became generally known. Nevertheless, the translators deserve praise for their devoted labors in bringing out a handsome book in a form in which the English-reading public may judge for itself the importance of Weber's contribution and, incidentally, the justice of the present appraisal of it.

GEORGE A. LUNDBERG

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THE GOOD EARTH

Forest Soils and Forest Growth. S. A. Wilde. xx + 241 pp. Illus. $5.00. Chronica Botanica. Waltham, Mass. G. E. Stechert. New York. 1946.

FOREST soils cover about one-half the total land area of the globe. "Forest soil is the medium that produces nature's most magnificent crop. . . .Since time immemorial the daily needs and inborn inquisitiveness of mankind have stimulated interest in the soils of the forest."

In *Forest Soils and Forest Growth*, Dr. Wilde has summarized his exhaustive review of the world's literature on forest soils. He brings to the reader all the rich store of accumulated Russian soil and forest lore. Probably nowhere else can be found in one book such a concentration of references, abstracts, and summaries of forest soil literature. In this book he has recorded also the essence of his years of experience in teaching and research in Europe and America.

One outstanding quality of this book is its clarity. The book is true to its title: The author leads the reader through the necessary applications of chemistry, physics, biology, hydrology, and ecology; but he retains coherence and unity by never straying far from the central theme, forest soils.

Another refreshing quality of the book is its courageous originality. The author does not hesitate to suggest constructive changes in, and additions to, present accepted usages. For the expression "humus incorporation into the mineral soil in the formation of a mull," he presents a new word, "melanize." Melanize is derived from the Greek word *melas*, meaning "black" or "dark." The sound of the word suggests mellow—the exact quality of a good mull soil.

The text is systematically arranged. From an introductory setting including the history of the rise of the science of forest soils and the general nature and importance of forest soils to silviculture and land use, the author develops his theme. Under Genesis of Forest Soils he explains the processes of melanization, podzolization, and gleization. The reader learns how a mull, a podzol, or a gley soil is formed and why each develops under prevailing conditions. The author explains the relation of climate, topography, and parent material to soil groups.

Forest soils are treated in two major groups, upland and hydromorphic. The author's separation of soils according to leaching and gleization makes the genetic soil groups of the world easily understandable.

At length, having classified the soil groups, the author explains the relations of trees to soil. Chapters on physical and on chemical properties of soil, soil organisms, and forest humus widen the reader's perspective.

The text moves from the simple essentials of a forest soil to the relations of soil to trees and of trees to soil and then to practical application of the science of soils to silviculture. The author clearly explains the necessity of forest soil surveys and site evaluation in advance of attempts at afforestation.

Finally, soils and tree planting, silvicultural cuttings, nursery practice, fertilizing, and parasitic control in forest nurseries complete the book.

Original drawings throughout this book make easy reading. Little woodland sketches at the chapter ends add an intimate artistic touch to the general excellence of the text.

JOHN T. AUTEN
Central States Forest Experiment Station
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Comments and Criticisms

ZENO AND THE MATHEMATICIAN[1]

Achilles had overtaken the Tortoise, and had seated himself comfortably on its back. . . .[2]

Mathematician: You here, Zeno? A pleasant surprise, indeed, as I can now ask you personally what you meant by your paradoxes. Was it jest or was it serious when you said a flying arrow could not move?

Zeno: Newcomers in eternity are always surprised—because they did not acquaint themselves with our philosophy when they were still in existence. I am just as real now as I was when my existence appeared to the experience of others; and whether my disappearance from the experience of others has taken place a long or a short time ago—a long or a short past with reference to their own times[3]—makes absolutely no difference as to my being. I am.

Mathematician: I would never have thought of that when I was on earth—I was so busy measuring changes, movements and velocities of bodies that I never had time to think—but here I am compelled to admit that you are; I could not have started this discussion with you if you were not. But now, what about your unquenchable paradoxes? I am eager to inform you that we mathematicians have solved them by means of the calculus of convergent series approaching the limit symbolized by zero; and instantaneous velocities are a commonplace in modern physics—we read our speedometers every day, you see. You must enjoy the news that your problem has been solved, even though we ourselves do not understand how we do it: whether there is an actual infinity of numbers in such a series and whether a limit is actually reached or not. Still, we do it. We have made progress. The motionless arrow only seemed a problem to you because mathematical methods were so crude in your time.

Zeno: You advanced mathematicians seem to me no less naive than the simple mathematicians of my time. They said the same thing and also solved the paradoxes *ambulando.* They simply did it. As Lewis Carroll, a good friend of mine, put it, they would better have taken to football than to thinking. I am delighted, of course, that your methods of measuring relative velocities are so nicely perfected now and I celebrate with you such improvements in this art. In Greek we call this *technē.* But my paradoxes, you remember, were written in defense of Parmenides. They were never merely mathematical riddles. If they were only that, they would not be paradoxes.

Mathematician: What is a paradox?

Zeno: Understand, friend, that in asking this question you move outside your calculating habits. And when I say "outside" I do not mean a geometrical proposition, but a different logical question. Now, if you have made your mind up beforehand that only mathematical riddles represent serious questions, then you had better abstain from pursuing this further. You are now warned!

Mathematician: I don't know what you are driving at. But for the fun of an argument, go ahead; I am ready to listen.

Zeno: That is something, but I am afraid it is not enough. Nevertheless, may I take your word that you are willing to consider my argument as an hypothesis?

Mathematician: Yes.

Zeno: Your "hypothesis," then, that I am, must be either true or false.

Mathematician: Do you mean to say that some people have verified your existence in their experience, and have talked about you?

Zeno: That is another hypothesis which must be true or false. How do you verify it?

Mathematician: I myself experienced that other people refer to you as having existed.

Zeno: That is another hypothesis. We need, it seems, quite a "number" of them. And the more you examine them in detail, the more prolific and the more uncertain they become. But take as many and make them as detailed as you please, do all of them prove that I am?

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[1] Court, Nathan. The Motionless Arrow. *Sci. Mon.*, **63**, 249, 1946.

[2] Carroll, Lewis. *What the Tortoise Said to Achilles.* Complete Works, 1225. Modern Library.

[3] Mueller, Gustav E. Experiential and Existential Time. *Philosophy and Phaenomenological Research*, **6**, (3), 1946.

Mathematician: Of course not. All of them only sum up to this that I read, that other people read, that still other people (including yourself) had an experiential impression of a man named Zeno.

Zeno: Yet all the while you tacitly presuppose that you and all the others and I myself *are*. And this ontological presupposition of all the many and fluctuating empirical hypotheses remains one and the same for all of us, regardless of whether we lived earlier or later in time or whether we found ourselves located in Elea, Italy, or in Norman, Okla. To verify this one and same Being experientially is impossible, because you and I and everybody must *be*, before we can start having experiences in time or before we can make abstract experiential calculations of temporal data. "Being" *logically* precedes temporal existence and temporal experience, and it is the one and same Being logically "in" all those times, take them as long or cut them as short as you want to. Being is "in" all experiential and existential movements but does not move.

Mathematician: You use undefined terms, and your argument, therefore, makes no sense to me.

Zeno: As long as you do not say it is nonsense, you sit perfectly safe in the dubitability of your earthly memories. Otherwise, if you say either "It is nonsense" or "Only my earthly memories are true," you would end up in saying that "I am" is nonsense—and that would be unconditionally paradoxical, don't you think?

Mathematician: I don't know since you have not defined your paradox.

Zeno: Excuse me, here comes Parmenides who maintains that experience is the seat of all errors. He may enlighten you better than I can who am only his admirer. May I introduce you? Venerable father, here is another one of those mathematicians who complains that we have not told them what the paradox is.

GUSTAV E. MUELLER

The University of Oklahoma

PARAPSYCHOLOGY

Only just yesterday did I get as far as "The Brownstone Tower" in your August number; and, as I pondered over Table 1, an idea, or what seemed to be such, struck me with sufficient force to induce me to write you this letter.

The idea is that your assumption that members prefer articles relating to their sections is not well founded. It seems to me that, in SM, they like to find what they can't get in their own technical journals. I am classified under M; but my interest in SM is not because I expect any engineering articles in it. Almost anything but engineering is what I am looking for; because SM, to me, is a source of general information not merely about various subjects but also about what people are thinking and doing about those subjects. Sometimes they are subjects that I did not even know had been conceived; and then I am indeed delighted.

An article that would especially interest me would be one on psychic research or, rather, *about* psychic research. I mean it would not be an investigation of "psychic" phenomena but an investigation of the investigation of such things. For, you know, there *is* something going on over there in that field; and I cannot satisfy myself that they are all crazy.

RUDOLPH H. KLAUDER

Ocean City, N. J.

ATOMIC WAR

Discussion of the atom bomb in connection with war sounds fantastic, and equally so ideas to avoid this catastrophe. Yet Russell and Wells have both said such a war will be the end of civilization as we know it; and within the month well-known men, including educators, have told youth there is no chance for a one-world government in this generation, one saying, not until after the next and possibly still another war. The ideas of control, if I am not mistaken, seem to lie in agreements to stop research, disclose mineral deposits, submit to routine inspection; and for us to get rid of our stockpile. Yet this is no solution. It merely strips us, on the word of others, and depends on whether or not we (and they) are clever enough to formulate a spy system to make it stick. So the fear will be constant. In this dilemma the old adage that "like cures like" may be recalled. Army ideas and plans of concealed forts bearing bomb-laden rockets, trained for instant dispatch on any world capital, are no secret. They were outlined in a recent popular magazine. If, therefore, it were known such exchange bases and messengers of destruction were constantly alert through the world, no one would be first to press the button, no matter what the provocation. Indeed, were a system of confirming the offender ready, perfectly worked out, all other nations could join in immediate retaliation, which would certainly settle matters for such a nation. We will have to meet the fear by accepting it boldly and equally with materialistic science—not words on paper. Nor would any prior agreement to such action be required. We could do it and let it be known, then give our data

to responsible United Nation members. A plan for knowing who pulls the switch first and when would thus become a matter of mutual interest, and quickly. I am unable to conceive of any other possible plan simply because, if we keep the secret, it will come out anyhow and leave us as naked, then, as any other nation. A drastic threat to the very existence of humanity requires heroic measures.

H. E. WOODCOCK

Chicago, Ill.

JUSTLY CYNICAL

While it is true that the experimental aspect of science and its data, theories, and applications justify *Homo sapiens* in possessing a fair amount of self-respect, any pride regarding a general understanding of Nature, or even a reasonable theory of it all, is a delusion of grandeur only.

It can be said without risk of serious contradiction that no published theory, hypothesis, assumption, or guess about the essence of Nature has ever shown better validity as a fundamental explanation than that once proposed by Arrhenius for the *origin* of life upon the earth, namely, that of being the result of germs having been wafted from some undermined outside source by the pressure of light. Every attempt to describe a creation or beginning for the universe or a galaxy starts out with assuming a *fait accompli*, and as for fundamental maintenance no explanation proposed on a materialistic basis has ever been plausible, even when advanced with great academic authority.

The not very ancient hypothesis of an electromagnetic ether associated with matter, as it was concurrently understood, is well within the memory of all but the youngest scientists. The theory at the time was not considered to provide for gravitation, although with the great gain in experimental evidence an ether could be inductively described that would do so. The mere fact that the electromagnetic ether hypothesis was difficult to handle mathematically in itself did not justify its abandonment. A simpler mathematical treatment of the data has lead to great confusion, complexity, and contradiction in major theories. It has been recently admitted by competent authority that no progress has been made toward a unified general theory in the past six years and, inferentially, that the trouble is not so much due to the intricacies of a hypothesis of a nonmaterialistic universe of potential superenergy with the abandonment of the present concepts of matter as such, as that the very character of existing mathematics disqualifies it in attempts to penetrate the present scientific "stone wall." It has been said that what is needed is another Newton and a new kind of mathematics. Let it be hoped that a possible newly organized logic of sufficient power to cope with the situation may not be handicapped by the name mathematics.

CHESTER B. DURYEA

Bay Shore, N. Y.

IMPORTANT BOOKS

May I point out a bibliographical inaccuracy in J. J. Schifferes' interesting article in the September issue of the SM? On page 237, under the heading "The Ten Most Important Books in the History of Science," he lists as item 3 the "*Novum Organum* (The New Atlantis). 1620, Francis Bacon. . . ."

This must be a slip of the pen, since the *Novum Organum* and the *New Atlantis* are different works. The *Novum Organum* (1620) is of great importance in the history of science since it is the first attempt to explain a kind of inductional method for the sciences. It is written in the form of a series of aphorisms but is really a systematic treatise. Much of it is devoted to a minute analysis of the method as applied to an investigation of the nature of heat. The *New Atlantis*, on the other hand, is a fragmentary Utopian romance, first published posthumously. Although it contains a brief fanciful sketch of how scientific research would be organized in the perfect state, it is of no importance whatever in the history of science.

RUFUS SUTER

Washington, D. C.

DISAPPOINTMENT

Frankly, and only with the thought of constructive criticism, I must tell you that I have been much disappointed with the editorial content of SCIENTIFIC MONTHLY. There is too much mediocre material in it and too much that requires a long stretch of the imagination to connect it with science.

With the scientific research laboratories of our great educational institutions and our various industries making real progress in science in many directions, there should be available a vast amount of material of really scientific nature to make a monthly publication worthy of this great Association. Your "Science on the March" is a fine title and suggests just what we want, but it does not begin to keep us posted on the progress of science.

J. M. WATSON

Takoma Park, Md.

The Brownstone Tower

THOSE who could not attend the recent Boston meeting of the A.A.A.S. probably read about it in the newspapers during Christmas week. About 60 science writers and reporters were in the press room at the headquarters hotel writing stories of the meetings from manuscripts and abstracts of papers that were submitted in advance by their authors. It would be interesting to know what these reporters chose to write about. They are skilled in recognizing stories of potential interest to the public, and their choice might help to guide the editor of the SM in his search for interesting articles. Because no arrangement was made with the reporters present to get this information, it would be possible now only to find out what the newspaper editors chose to publish of the copy submitted to them. It would be interesting also to analyze the geographical distribution of the stories on the meetings in cities of, let us say, more than 100,000 population, in terms of total space allotted and that given to certain stories. No doubt the meeting was most fully reported in Boston, but what of other cities?

I had hoped to make some such investigation in time to report my findings in this issue, but was too busy to do it. A principal article may be written on this subject later.

Aside from conversations with old friends and contributors to the SM, I had two memorable experiences: First, I heard Admiral Parsons talk about Operation Crossroads at Bikini, certainly the most elaborate and spectacular experiments ever made, and I saw the Kodachrome movies of the two tests. Second, I attended a meeting of the A.A.A.S. Council in the Faculty Room of University Hall, which stands in the center of the Harvard Yard. The Yard was covered with crusted snow that night. Arriving early, I walked alone through that Colonial campus, the darkness broken here and there by house lights and the gleaming ice near them. It was a privilege to see the old faculty room of Harvard. There were the likenesses in portrait and statuary of many of Harvard's great men, including two of her famous scientists, Asa Gray and Louis Agassiz. President Conant had only to walk across the Yard from his home to preside at the meeting at which another great Harvard man, Harlow Shapley, was elected president of the A.A.A.S. for 1947. Harvard and Yale divided the honors that night: Shapley and Mather of Harvard for president and member of the Executive Committee, and Sinnott and Baitsell of Yale for president-elect (1948) and member of the Executive Committee, respectively.

The retiring president, Dr. C. F. Kettering, called attention to the SM in his address at Symphony Hall. He said:

> It is often just as important for the business man, the lawmaker, and the general public to know of discoveries in the field of science as it is for other scientists to know of the technical details. . . . Science will be advanced furthest when its results and problems are better known and understood. *Our* SCIENTIFIC MONTHLY publishes less than 15,000 copies. . . . We should aim at a circulation of several hundred thousand.

Note the word "our," which I have italicized. If the members of the A.A.A.S. will regard the SM as their own medium for reaching the public, if they "can or will write it so that the average man or student can understand," we shall be on our way to the circulation envisaged by Dr. Kettering and the consequent advancement of science among the educated public.

F. L. CAMPBELL